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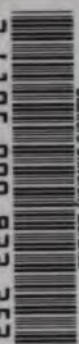
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LEANT BRAYTON: A LION OF THE

PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From November 21, 1901, to February 27, 1902.

VOL. LXIX.

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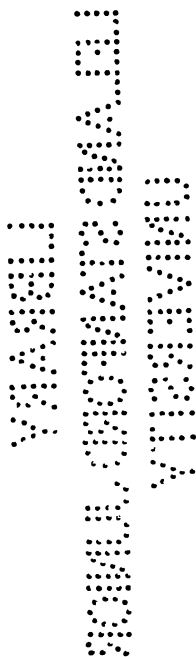
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"A Determination of the Value of the Earth's Magnetic Field in International Units and a Comparison of the Results with the Value given by the Kew Observatory Standard Instruments." By W. WATSON, B.Sc., F.R.S., Assistant Professor of Physics at the Royal College of Science, London. Received June 6 — Read June 20, 1901.

(Abstract.)

A comparison of the readings given by the magnetometers used for measuring the horizontal component of the earth's magnetic field at the various observatories in Great Britain having shown the existence of very marked discrepancies, although the instruments employed are all of the same type, it seemed of interest to employ some entirely different method for the measurement of H , and to compare the results with those given by the unifilar magnetometers. This paper contains a description of such a measurement. The method employed consists in passing a known current through a coil of known dimensions and comparing, by means of a suspended magnetic needle, the field at the centre of the coil, due to the passage of the current, with the horizontal component of the earth's field. The current employed has been measured in two ways:—(1) by the deposition of silver in a voltameter, and (2) by a potentiometer method, in which the difference of potential at the terminals of a known resistance is balanced against the E.M.F. of a standard Clark or cadmium cell.

As a result, it has been found that taking the electro-chemical equivalent of silver as 0.001118, the value of H as determined by this galvanometer method is 0.00006 C.G.S. unit lower than the value given by the Kew Observatory standard instrument. If, as seems probable, the true value of the electro-chemical equivalent of silver is 0.0011183 then this difference is increased to 0.00014 C.G.S. unit.

"On the Composition and Variations of the Pelvic Plexus in *Acanthias vulgaris*." By R. C. PUNNETT, B.A., Gonville and Caius College, Cambridge. Communicated by H. GADOW, F.R.S. Received February 16,—Read March 7, 1901.

Whilst recently engaged upon the development of the pelvic plexus in *Mustelus levis* I was much struck with the amount of variation in the number and position of the nerves forming it. This led me at the time to examine all the material which I was able to procure, and the results obtained have been given in a previous paper (12, 1900). Believing, however, that, in Bateson's words, "the facts of variation must be the test of phylogenetic possibility" (4, p. 30), I determined to examine a still larger number of specimens of some other species of shark. The only two species in this country which from their abundance lend themselves to such an investigation are *Scyllium canicula* and *Acanthias vulgaris*. The former possesses certain advantages in that it would be possible to institute comparisons with the nearly allied *S. catulus* which co-exists with it in the same locality. Such advantages, however, are to some extent discounted by the greater difficulties which *Scyllium* presents in working. Owing to the greater abundance and toughness of the connective tissue it is not nearly such a favourable object for investigation of this kind as *Acanthias*. This is also the case with the embryos—those of *Acanthias* giving far clearer preparations with the gold chloride method mentioned below than the *Scyllium* embryos. The presence of a large "nervus collector" in *Acanthias* also gives it an advantage over *Scyllium* where the "collector" is usually represented only by a single branch.

The research was carried on at Plymouth in the summer of 1899 and of 1900, whilst occupying at the Laboratory tables both from the British Association and from Cambridge University. To the authorities of the Biological Station I would express my thanks for the ample supply of material forthcoming. I also take this opportunity of expressing my gratitude to Dr. Gadow for the kind sympathy which he has shown during the course of the work.

The anatomy of the pelvic plexus of *Acanthias vulgaris* has been studied by Davidoff (1) and by Braus (10). Davidoff's account may be briefly summarised as follows:—

The pelvic fin is innervated by seventeen nerves whose serial numbers are 31—47. Of these nerves 40—47 each divide on reaching the fin, into a dorsal and a ventral portion which innervate respectively the fin musculature lying dorsally and ventrally to the fin skeleton. Before supplying the muscles, however, the ventral branches all run into a longitudinal nerve, lying at right angles to their direction

(ventraler Längsstamm) from which the nerves to the muscles are given off. The dorsal branches behave similarly, but the arrangement is slightly more complex owing to the fact that two such longitudinal nerve stems are found, the "medialer" and the "lateraler Längsstamm."* The ventral branch of the 40th nerve passes through the more medially situated of the two pelvic foramina before joining the longitudinal stem. The dorsal and ventral branches of nerves 31—39 are indistinguishable along the greater part of their course, as they all run together in a long nerve stem—the nervus collector—until just before the point where the girdle is reached. Here the dorsal branches contained in the collector separate from the ventral branches. The former are continued as a branch which runs into the dorsal "Längsstamm," whilst the latter pierces the more laterally situated of the two pelvic girdle foramina and enters the ventral "Längsstamm." The specimen described by Braus differs considerably, since (1) the nerves entering the fin are 25—45, (2) the girdle is pierced only by the 36th nerve which apparently corresponds to Davidoff's 40th nerve, and (3) the ventral branches of the collector do not pass through a foramen. These two cases alone are sufficient to show that a considerable amount of variation occurs.

The object of this investigation was to ascertain the range of such variations, and to endeavour to find out whether they afford any support or otherwise to either of the two theories which attempt to explain the varying position of the limbs in different species and in different members of the same species, *i.e.*, the theory involving vertebral inter- or excalation and the theory of limb-migration. For this purpose a number of individuals of both sexes was examined, and the following points in each specimen accurately determined:—

- (a) The serial number of the girdle-piercing nerves.
- (b) The number of nerves forming the collector.
- (c) The number of post-girdle nerves.
- (d) The point of junction of the whole- and half-vertebræ ("Ganz- und Halb-Wirbel").

For the determination of the serial number of the nerves piercing the girdle the arrangement of the occipital region was in each case examined. This was found to be practically constant, thus affording a fixed point from whence to reckon. The nomenclature adopted for the nerves of this region is shown in fig. 1.†

The determination of the point of junction of half- and whole

* Cf. *loc. cit.*, Taf. 29, fig. 15, and Taf. 31, fig. 27.

† This nomenclature has been adopted for convenience, and it does not necessarily follow that the nerve here denoted as *z* is homologous with the nerve similarly designated in other forms by Fürbringer and others. As Fürbringer's Memoir (8) is not accessible, I am unable to say at present whether for this species his *z* and my *z* are identical.

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vertebræ presented somewhat more difficulty. Over each centrum the trunk, and towards its caudal end, issue the dorsal and ventral roots of a spinal nerve—the latter being rather more rostrally situated than the former. As the nerve whose ventral root issues between

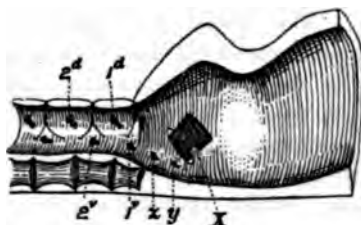


FIG. 1.—Hinder portion of skull and first few vertebræ. Seen after a new median sagittal section.

skull and first vertebra has been taken as z , it follows that the first spinal nerve will issue out over the first centrum, the second nerve over the second centrum, and so on. This relation is always kept as we pass caudally along the vertebral column, *i.e.*, a nerve of a given serial number always leaves the column over the caudal end of a centrum having the same serial number. When, however, we reach a point on the vertebral column somewhere between the 45th and 50th

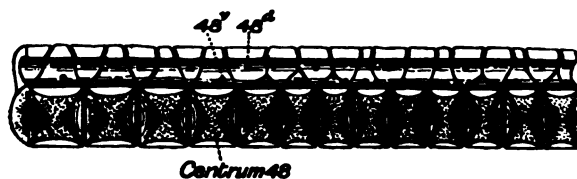


FIG. 2a.

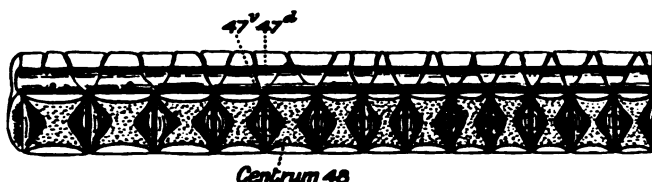


FIG. 2b.

Two vertebral columns at region where the transition from half- to whole vertebra occurs. Seen from the inside after bisection and removal of dorsal cartilages of one side.

centrum we suddenly meet with a centrum over which there issues no spinal nerve (cf. fig. 2a, centrum 49). This centrum has been always reckoned as the first half-vertebra.

Of course it might be argued that in this particular instance centra 48 and 49 constitute the first two half vertebrae, and not centra 49 and 50; and similarly for all other cases.

Judging from analogous cases, such as that of *Amia calva* (Gadow 6, p. 202 and fig. 14), this latter view may possess rather more support. Until the question is settled by the investigation of the innervation of the myomeres in this region more cannot be said. The point of importance here is that a uniform system of nomenclature has been adopted for all the specimens examined. It may be mentioned that the mere size of the centra does not constitute an accurate indication of this point of junction. Though the first "halb-wirbel" centrum is usually smaller than that of the last "ganz-wirbel," yet a glance at fig. 2b will show that this is not always so. In this case centra 48 and 49 belong to the first half-vertebra in spite of the fact that centrum 49 is at least as large as any centrum of the whole vertebrae.

As the collector nerve lies along the outer side of the vena parietalis, injection of this with osmic acid rendered visible even the minutest branches which went to form it. The post-girdle nerves were also dissected with the help of the same reagent, which showed them up very readily in the fresh material used.

One hundred specimens in all were examined, of which fifty were males and fifty females. Of the males twenty-nine were adults ranging from 35 cm. to about 90 cm. in length, whilst forty of the females ranged between these sizes. The rest were old embryos about 25 cm. long, very nearly ready to be born, which for all intents and purposes, as will be seen below, could be reckoned as adults. The tabulated results of the points examined are given in Table I.

Table I. Record of Points investigated in each specimen.
 Nos. 1—50 are males; Nos. 51—100 females. * denotes that the specimen is an older embryo.

No. of specimen.	Girdle-piercing nerves.	Post-girdle nerves.	No. of nerves to collector.	Whole vertebræ
1	35	10	9	45
*2	35-36	10	11	46
3	35-36	10	11	46
4	35-36	10	11	46
*5	35-36	10	11	47
*6	35-36	10	10	46
7	35-36	10	9	45
8	35-36	10	9	45
9	35-36	10	9	46
*10	{ 35-36 36-37 }	11 10	{ 12 13 }	46
11	{ 36 36-37 }	9	12	46
*12	36-37	11	12	48
*13	36-37	11	11	46
*14	36-37	11	9	48
15	36-37	10	11	48
16	36-37	10	11	47
17	36-37	10	11	47
18	36-37	10	11	47
*19	36-37	10	10	49
20	36-37	10	10	48
*21	36-37	10	10	47
*22	36-37	10	10	46
*23	36-37	9	12	47
*24	36-37	9	11	47
25	36-37	9	11	46
26	36-37	9	10	47
*27	36-37	8	11	46
*28	36-37	7	10	48
*29	37	9	10	47
30	37	9	10	47
31	37	9	10	47
32	37-38	10	11	46
*33	37-38	10	11	47
34	37-38	10	11	47
35	37-38	9	12	46
36	37-38	9	11	48
*37	37-38	9	10	47
38	37-38	9	10	47
*39	37-38	9	10	48
*40	37-38	9	10	48
41	37-38	8	12	46
*42	38	7	10	48
43	38-39	10	12	47
44	38-39	10	9	48
45	38-39	9	12	48
46	38-39	9	12	48
47	38-39	9	12	48
48	38-39	9	11	47
*49	38-39	9	11	51
50	38-39	8	{ 13 12 }	47
51	35-36	9	9	46

Table I—continued.

No. of specimen.	Girdle-piercing nerves.	Post-girdle nerves.	No. of nerves to collector.	Whole vertebræ.
52	35-36	9	10	47
53	36-37	7	12	46
54	36-37	7	11	46
55	36	7	11	47
56	36	8	10	46
57	36-37	8	11	47
58	36-37	9	11	46
59	36-37	9	10	46
*60	36-37	9	11	47
61	36-37	9	9	47
62	36-37	10	9	45
63	36-37	10	10	46
64	{ 36-37	10	10 }	46
	{ 37	9	11 }	
65	37	6	12	45
*66	37	7	11	46
67	37-38	8	11	46
68	37-38	8	10	46
69	37-38	8	11	47
70	37-38	8	11	47
71	37-38	8	11	47
72	37-38	8	10	47
73	37-38	8	12	48
*74	37-38	8	10	48
*75	37-38	8	11	49
76	{ 37	8	13 }	
	{ 37-38	9	12 }	46
77	37-38	9	9	47
78	37-38	9	12	49
*79	37-38	10	11	48
80	38	7	11	46
81	38-39	7	11	47
82	38	7	10	47
*83	38	7	9	47
84	38	7	11	48
*85	38-39	7	9	48
86	38-39	8	9	46
87	38	8	10	47
88	38	8	10	47
89	38	8	12	48
*90	38-39	8	11	49
91	38-39	8	11	49
92	38-39	9	11	48
*93	38-39	9	11	50
94	38-39	10	11	49
95	{ 38-39	8	11 }	
	{ 39	7	12 }	48
96	39	7	12	48
97	39	7	12	48
98	39	8	10	50
*99	{ 39-40	7	10 }	
	{ 40-41	6	12 }	50
100	{ 39-40	7	12 }	50
	{ 40			

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The range of variation with regard to the following points will be first examined, after which it is proposed to find out whether any correlations between the different parts can be established.

- I. The serial number of the first girdle-piercing nerve.
- II. The number of the post-girdle nerves.
- III. The number of nerves forming the collector.
- IV. Asymmetrical variations.
- V. The number and position of the nerve canals.
- VI. The number of the fin rays.
- VII. The number of the whole-vertebræ.

I. *Variations in the Serial Number of the First Girdle-piercing Nerve.*

According to Davidoff's account the pelvic girdle is pierced by two foramina, through which pass the ventral elements of two consecutive fin nerves. Of these two foramina the more laterally situated is the larger, and affords a passage to the ventral elements of the nervus collector. Variations occur in this arrangement as will be seen below. In this paper the last nerve taking part in the formation of the nervus collector has always been regarded as the "first girdle-piercing nerve," though occasionally, as will appear below, the foramen through which it passes is no longer complete, but is reduced to a groove. The sub-joined table shows the varying position of this nerve in the two sexes.

Table II.

Serial number of first girdle-piercing nerve.	♂.	♀.	
			Average for ♀ 37·25
			" " ♂ 36·43
			Diff. . . . 0·82
			(♂ being more rostral.)
35	19	4	
36	37	23	
37	26	31	
38	18	31	
39	—	9	
40	—	2	

Since the serial number of the first girdle-piercing occasionally differs on the two sides of the same individual, each side has been given separately as an individual case. Consequently the 100 cases for each sex in the above table are taken from fifty specimens respectively. The chief point of interest here is that the males show a marked tendency to a more rostral position of the girdle, as compared with the females. This amounts to $37·25 - 36·43 = 0·82$, or nearly a whole metamere.

II. *Variations in the Number of the Post-girdle Nerves.*

A considerable amount of variation occurred under this head, and the amount shown by the two sexes differs—the number in the male being greater than in the female. The results may be tabulated as follows:—

Table III.

Number of post-girdle nerves.	♂.	♀.		
6	—	3	Average in ♂	9.45
7	4	26	" " ♀	8.06
8	6	38	Diff.	1.39
9	38	24		
10	45	7		
11	7	—		

From this table it appears that there is a greater caudal extension of the fin innervation area in the male, which amounts to 1.39 metamere. It has, however, already been seen that in the male the pelvic girdle is on the average 0.82 metamere, more rostrally situated than in the female. Consequently the nett caudal preponderance of the fin innervation area in the male over that in female amounts to $1.39 - 0.82 = 0.57$ metamere.

III. *Variations in the number of Nerves forming the Collector.*

Table IV.

Number of nerves forming collector.	♂.	♀.		
9	12	14	Average for ♂	10.70
10	30	24	" " ♀	10.69
11	36	42	Diff.	0.01
12	20	19		
13	2	1		

This table shows that, whilst the number of the nerves forming the collector is subject to a considerable amount of variation, there is a striking agreement in the average number of collector nerves in the two sexes.

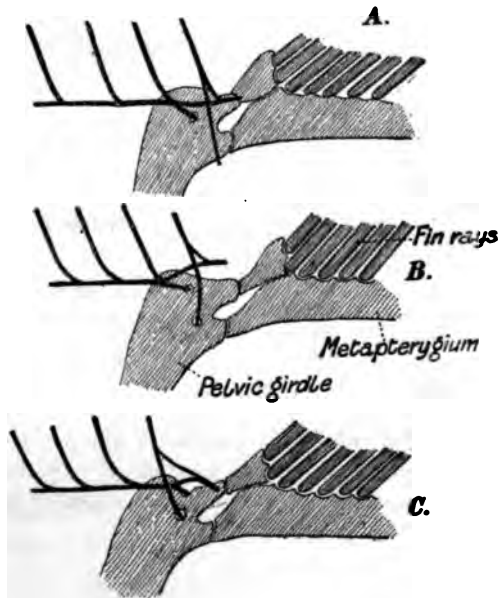
IV. *Asymmetrical Variations.*

In five out of the hundred cases examined the pelvic girdle was found to be asymmetrically placed (*vide* Table I, Nos. 10, 64, 95, 100), that is to say, the serial number of the first girdle-piercing nerve differed on the two sides. I have already shown that the same phenomenon occurs in *Mustelus laevis* ((12), pp. 335 and 343), and in other groups it has been noticed more especially by Paterson ((1), p. 522) in the case of man, and by Bumpus ((7), p. 465), and Ward ((9), p. 79) in the case of *Necturus*. The last two writers suggest that in *Necturus* such asymmetry may be due to the fact that the curvature of the developing Amphibian embryo is lateral, seeking to account in this way for the large number of asymmetrical variations in the group, which amounts to 8 per cent. Much stress, however, cannot be laid on this, as Paterson has shown that asymmetrical variations reach as much as 8·3 per cent in man, whilst among Elasmobranch the percentage reaches 5 per cent, both in *Mustelus* and *Acanthias*. Both these last groups differ from *Necturus* in that their embryonic curvature is dorso-ventral instead of lateral.

V. *Variations in the Number and Position of the Nerve Canals.*

It has already been stated that there may be either, one or else two nerve canals in the pelvic girdle. These variations may be arranged in three classes—fig. 3, A, B, and C.

FIG. 3.



In A there is only one foramen, and through it pass the ventral branches of the nervus collector. In type B there are two foramina. Through the more lateral one pass the ventral branches of the collector, whilst the more median and posterior foramen gives passage to the nerve immediately succeeding the collector. This arrangement is the most usual one, and corresponds to that figured by Davidoff. In type C there is but one foramen, through which passes the ventral portion of the nerve succeeding the collector. The ventral division of the collector itself passes now through a more or less pronounced notch on the lateral border of the girdle.

Now according to the views of those who uphold the "side-fold" theory of limb origin, and explain the varying position of the girdle by assuming inter- or ex-calcation of vertebræ, the pelvic girdle has been derived from the hypothetical segmental blocks of cartilage which were formed in the side-fold. As some of these coalesced and grew in medially to form the girdle they came to enclose the nerves which originally ran down between them. Consequently we may look to the nerves piercing the girdle as some guide to the number of morphological units which, on this theory, go to make up the girdle.

According to this view the girdle in *Acanthias* is composed of at least three such units (fig. 4, x , y , z). And to fit the facts it must be assumed that either x or z is capable of being dropped out. Consequently this theory involves the necessity of supposing that the pelvic

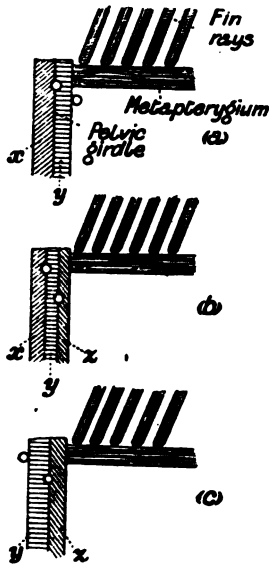


FIG. 4.

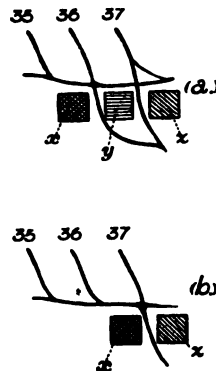


FIG. 5.

girdle is not homologous throughout the members of the same species, but that two-thirds of the morphological elements of the girdle are capable of undergoing some process of absorption. The capability of being absorbed possessed by the two outer elements of the girdle (x and z) will not, however, explain those instances where the girdle is asymmetrically situated. Take for example No. 64 (Table I). On one-side (fig. 5 (a)) the girdle is pierced by the 36th and 37th nerves. On the other side it is pierced only by the 37th (fig. 5 (b)). Now on the side-fold excalation theory the 37th and 36th nerves should bear the same relation to the three elements of the girdle. This can only be so if we assume that the middle element y has been dropped out. Consequently, in order to bring the theory into line with the different variations presented by the pelvic foramina and the nerves piercing them, we must assume that any of the three postulated elements of the girdle are capable of undergoing absorption, and, as has already been mentioned, we must further assume that a girdle which is pierced by one nerve is not homologous with a girdle pierced by two nerves. Also that a girdle pierced by one nerve is not necessarily homologous with any other that is also pierced by a single nerve (cf. fig. 4 (a') and (c)).

If, on the other hand, we seek to explain such variations on the migration theory we are not met with such difficulties. We may assume that the pelvic girdle in one specimen is strictly homologous with the girdle in any other specimen. We may suppose that the presence of one or of two nerve canals is determined by the position in which the "Anlage" of the girdle happens to be laid down. Thus in fig. 3, if we assume, for the sake of argument, that the most caudal nerve shown in each diagram is homologous, type C would result, if the "Anlage" of the girdle were laid down, in the most caudal of the three positions shown. If the "Anlage" be laid down a little more rostrally we should get type B; still more rostrally, and type A would result. Though it is possible that there may be some ontogenetic migration,* yet the facts are capable of explanation on this theory without the assumption of such a process. The matter has been discussed by Waite ((9), p. 86), who considers that in *Necturus* "the abnormal position of the girdle represents the development of a *new* girdle at a *new* point." The idea there discussed is similar to that given here with the exception of the *new* girdle. There seems to be no valid reason for supposing that the girdle in all cases is not a strictly homologous structure.

VI. *Variations in the Number of Fin Rays.*

The number of fin rays is subject to some variation. In order to ascertain whether there exists any correlation between the fin rays

* Cf. Braus (11), p. 588.

and the number of nerves innervating the fin—a correlation which on the side-fold excalation hypothesis we might be led to expect—I counted the fin rays in a dozen specimens of females. A small + after the number of fin rays denotes that it was uncertain whether an additional propterygial ray existed or not, the element in question being exceedingly small.

The result of the examination is given below :—

Table V.

Number of specimen in Table I.	Number of fin rays.	Number of nerves to fin.
59	16	19
55	16	18
63	16	20
61	16 +	18
52	17	19
71	17	19
89	17 +	20
51	18	18
56	18	18
80	18	18
81	18	18
95	19	19

A glance at the above table will show that there exists apparently no correlation between the number of fin rays and the number of nerves to the fin. Perhaps, however, it is best not to lay too much stress on this evidence, as the number of specimens examined is not very large.

VII. Variations in the Number of whole Vertebrae.

Mention has already been made of the way in which the point of junction between the half and the whole vertebrae was determined. The subjoined table gives the number of the whole vertebrae in the fifty specimens of each sex.

Table VI.

Number of whole vertebrae.	♂.	♀.	Average for ♂ 46·98 " ♀ 47·28 Diff. 00·30
45	3	2	
46	14	14	
47	18	15	
48	13	10	
49	—	5	
50	1	4	

This table shows that on the average the male possesses 0·3 of a whole vertebrae less than the female. It has already been shown that the position of the pelvic girdle in the male is on the average 0·82 metamere more rostral than in the female. The interesting question of the correlation between the number of whole vertebrae and the position of the pelvic girdle, will be examined in more detail later on in this paper.

In bringing these statistics to a close, the question suggests itself— which of the two sexes is the more variable? To attempt to answer this, I have calculated the standard deviation (σ) and also the probability error (E) for the two sexes on the following points:—

Table VII.

	No. of cases in each sex.	δ .		ϕ .	
		σ .	E.	σ .	E.
Number of collector nerves	100	0·99	$\pm 0\cdot066$	0·96	$\pm 0\cdot064$
Number of post-girdle nerves.....	100	0·86	$\pm 0\cdot058$	0·96	$\pm 0\cdot064$
Number of whole vertebrae	50	0·98	$\pm 0\cdot093$	1·29	$\pm 0\cdot122$
Serial number of 1st girdle-piercing nerve.....	100	0·99	$\pm 0\cdot066$	1·09	$\pm 0\cdot073$

From this table it will be seen that the variability of the female is greater on three of the above points, viz., with regard to (1) the serial number of the first girdle-piercing nerve, (2) the number of post-girdle nerves, and (3) the number of the whole vertebrae; whilst on the remaining point—the number of the collector nerves, the male shows a slightly greater variability, though here the two sexes are almost on an equality. Of course it must be borne in mind that the numbers dealt with are small for applying this method. Still the differences in the two sexes are fairly well marked, particularly in the case of the whole vertebrae, where the standard deviation in the female preponderates even when the maximum allowance is made in each sex for the probable error.

Having now concluded these statements on the amount of variation involved on the various points enumerated, we shall next enquire into the possibility of establishing any correlations between the position of the girdle and other points of variation.

A. *Between the First Girdle-piercing Nerve and the Number of Collector Nerves.*

As the nerves which take part in the formation of the collector are *only concerned in the innervation of muscles connected with the most*

rostral portion of the fin, it has not been thought necessary to consider the two sexes separately on this point. The following table shows the way in which the variations in the position of the first girdle-piercing nerve are associated with the variations in the number of the collector nerves.

Table VIII.

1st girdle-piercing nerve.	Collector nerves.					Total.	Average.
	9.	10.	11.	12.	13.		
35.....	10	4	8	1	—	23	10·00
36.....	6	19	26	8	1	60	10·65
37.....	2	20	23	11	1	57	10·80
38.....	8	8	21	11	1	49	10·79
39.....	—	3	—	6	—	9	11·33
40.....	—	—	—	2	—	2	12·00

From this table it will be at once seen that the more rostral position of the girdle is on the whole correlated with a reduction in the number of nerves taking part in the formation of the nervus collector. The only break in the ascending series of the average number of collector nerves as the girdle becomes more caudal, occurs where the girdle is pierced by the thirty-seventh and thirty-eighth nerves.

Here the average number of collector branches is practically identical in the two cases. The criticism may be made that a number of old embryos are included in the above table, and that, if we may judge from the case of *Mustelus* ((12), p. 335), these may show a larger number of collector branches than the adults, and so tend to bring irregularities into it. Such a criticism may be disposed of by the following two tables, in which a comparison is made between such embryos and the adults.

Table IX.

No. of collector nerves ..	9.	10.	11.	12.	13.	Average.
Embryos.....	6	23	26	6	1	10·56
Adults.....	20	31	52	33	2	10·75

Table X.

First girdle-piercing nerve.	35.	36.	37.	38.	39.	40.	Average.
Embryos	7	23	18	32	1	1	36·67
Adults	16	37	39	37	8	1	36·61

These tables show that, whilst the serial number of the girdle piercing nerve is identical, the average number of collector branches slightly less in the embryos than in the adults.

Consequently, for practical purposes, such embryos may be considered as being of equal values with the adults. From analogy with the case of *Mustelus* quoted above, it might have been expected that these older embryos would have shown a slightly larger number of collector nerves than the adults. It must, however, be borne in mind that these embryos of *Acanthias* are rather more advanced than those of *Mustelus*, which were previously considered. As far as I have been able to observe, an *Acanthias* embryo of a given length about this stage is more advanced than a *Mustelus* embryo of the same length.

Returning now to Table VIII., if we are to explain the correlation there expressed on the side-fold excalation theory, we must suppose that the lesser number of collector branches associated with a more rostral position of the girdle is due to excalation of vertebræ through which pass nerves to the collector as well as vertebræ in the pre-collector area. This will be made clearer by the consideration of a concrete case. In fig. 6 two cases have been selected, one in which

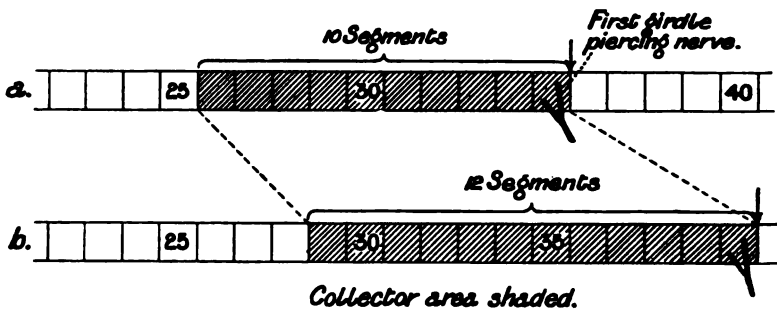


FIG. 6.

the first girdle-piercing nerve is thirty-five, and the other in which it is forty. From Table VIII. we learn that the average number of collector nerves associated with these two positions of the girdle are ten and twelve respectively. Now if we are to derive the case in which the girdle is more rostrally situated from that in which its position is more caudal, the figure shows that we must assume two segments to have been excalated in the collector area, and three in the pre-collector portion of the trunk. The assumptions made are (1) that the girdle-piercing nerve is homologous in each case, and (2) that a more rostral position of the girdle is in itself no argument for a lessened collector area. Consequently we must assume that on the *side-fold excalation theory*, excalation takes place both in the collector and the pre-collector areas.

On the migration theory, we should expect to find the more rostral position of the girdle associated with a lessened number of collector nerves. The reduction of the collector nerves, however, does not keep pace with the more rostral position of the girdle. From fig. 6 we see that with a change of five segments in the position of the girdle, there is associated a change of only two segments in the collector area. To explain this, we must suppose that those segments which lie just rostral to the collector area in the case where the girdle is more caudally situated, are capable of taking part in the collector when the girdle is shifted more caudally. Evidence in favour of such a view will be given later, when it is shown that in young embryos, the number of nerves forming the collector is greater than in the case of the adults (*vide* p. 19).

B. Correlation between the First Girdle-piercing Nerve and the Post-girdle Nerves.

It has already been seen (Table III) that on the average, the number of post-girdle nerves is considerably greater in the male than in the female. Consequently it has been found necessary to consider the two sexes apart on this point. The way in which the number of post-girdle nerves varies with the position of the girdle is shown in the following two tables:—

Table XI.

1st girdle-piercing nerves.	Post-girdle nerves 6.	7.	8.	9.	10.	Average.
35	—	—	—	4	—	9.00
36	—	6	4	10	3	8.43
37	2	2	19	6	2	8.13
38	—	12	13	4	2	7.87
39	—	7	2	—	—	7.33
40	1	1	—	—	—	6.5

Table XII.

1st girdle-piercing nerves.	Post-girdle nerves 7.	8.	9.	10.	11.	Average.
35	—	—	—	18	1	10.05
36	2	2	10	17	6	9.62
37	—	2	18	6	—	9.16
38	2	2	10	4	—	8.88

The tables bring out clearly the fact that, in both sexes, as the girdle comes to occupy a more rostral position, the number of post-girdle nerves shows a steady and marked increase. This is exactly what, on the migration theory we should expect to find. On the side-fold excalation theory, however, we should expect the position of the girdle to have no effect on the number of the post-girdle nerves. True, contraction of the fin may be going on, but even then there is no reason why the fins which show the greatest amount of contraction should be so regularly associated with a more rostral position of the girdle. The only way in which these facts can be explained on the excalation theory, is by assuming that the pre-girdle excalation which results in a more rostral position of the girdle, is accompanied by a post-girdle intercalation which leads to a greater number of post-girdle nerves. The greater the amount of pre-girdle excalation—the greater must we suppose is the amount of post-girdle intercalation.

C. Correlation between the First Girdle-piercing Nerve and the number of whole Vertebræ.

That there is a close correlation between the position of the girdle and the point where the whole vertebræ end is brought out by the following table:—

Table XIII.

1st girdle-piercing nerve.	Girdle whole 45.	46.	47.	48.	49.	50.	51.	Average.	Average number of post-girdle whole vertebræ.
35	6	13	4	—	—	—	—	45·91	10·91
36	2	24	22	10	2	—	—	46·76	10·76
37	2	15	24	12	4	—	—	47·02	10·02
38	—	4	16	19	6	2	2	47·83	9·83
39	—	—	—	5	—	4	—	48·88	9·88
40	—	—	—	—	—	2	—	50·00	10·00

From this table it will be seen that there is usually an interval of ten segments between the vertebra through which passes the first girdle-piercing nerve, and the last whole vertebra. This is especially well marked in the four most caudal positions of the pelvic girdle, *i.e.*, when the serial number of the girdle-piercing nerve is 37, 38, 39, or 40, the average number of whole vertebræ is exactly or very nearly 47, 48, 49, and 50 respectively.

Having just seen the necessity of assuming post-girdle intercalation to explain the larger number of post-girdle nerves associated with a

more rostral position of the girdle on the side-fold theory, we might expect to find some trace of this in the number of post-girdle vertebræ—that is, we might reasonably look for a larger number of whole vertebræ behind the girdle-piercing nerve when the girdle is situated more rostrally than when it is more caudally placed. A glance at Table XIII will show that this is only to a very small extent the case. Whilst the girdle shows a difference in position amounting to five segments, there is a difference of less than one between the average number of post-girdle whole vertebræ associated with its extreme positions.

On the migration theory such difficulties are not encountered. The comparative constancy in the number of post-girdle whole-vertebræ for all positions of the girdle is to be explained by supposing that there exists some relation between the position of the girdle and the point where that more flexible portion of the body—the tail with its half-vertebræ commences. The position where the “Anlage” of the pelvic fin is laid down must be supposed to determine, probably for mechanical reasons (*cf.* Gadow (6), p. 195), the point where the half-vertebræ shall start. Moreover, supposing the migration to be secondarily in a rostral direction helps us to understand why the number of post-girdle whole-vertebræ tends to be rather greater for the most rostral positions of the girdle, than for the intermediate or most caudal positions. We must imagine that the conversion of whole into half-vertebræ tends on the whole to keep pace with the rostral migration of the girdle. As the migration of the girdle attains its maximum the formation of half-vertebræ tends, so to speak, to lag behind, and as a consequence we find that in the most rostral positions of the girdle a rather larger average number of post-girdle whole-vertebræ.

Some Embryological Data.

The material used consisted of *Acanthias* embryos which had been preserved in corrosive sublimate and acetic acid. Horizontal longitudinal sections were cut through the pelvic area. These were treated with gold chloride and formic acid as described in a previous paper ((12), p. 344). Such portions of the plexus where the inter-communications of the nerves was required, were re-constructed on millimetre paper. The results may be tabulated as follows:—

Length and sex of specimen.	No. of nerves forming collector.	No. of nerves piercing girdle.	No. of post-girdle nerves.	No. of nerves forming posterior collector.
A. 31 mm. (?)	14	2	12	3
B. 32 „ (?)	13	2	11	3
C. 42 „ (?)	12	{ 1 on left 1 on right }	11	3
D. 50 „ ♀	—	2 (1st girdle-piercing nerve on left by groove)	10 (last small)	3
E. 62 „ ♂	—	2	11	3 } Most caudal branches reduced except in F
F. 66 „ ♂	—	2	11	2 }
G. 68 „ ♀	—	2	11 { last two very small	3 }
H. 72 „ ♀	—	2	11	3 }

In the younger stages, A—C, it is not possible to determine the number of nerves forming the collector as the mixipterygium is not yet distinguishable. The table brings out the following facts:—

- (1.) The number of nerves forming the nervus collector is greater in younger embryos or adults. This has also been shown by Braus for *Spinax niger* (11), p. 620, and by myself for *M. laevis* (12), p. 347).
- (2.) The number of post-girdle nerves is greater in younger embryos than in older embryos or adults (*cf.* Table III).
- (3.) The last two or three post-girdle nerves form a posterior collector (*cf.* (12), p. 347, and (11), p. 566), a structure which is separate from the “Langsstämme” occurring in the older embryos. These latter structures also are found in embryos of 45 mm. and upwards. In the two younger embryos (A and B) the posterior collector is not yet fully formed (*cf.* fig. 7). I have only observed the posterior collector in one case among older embryos and in adults. In the older embryo in which it occurred there were eleven post-girdle nerves, and the two branches of the posterior collector were extremely fine.
- (4.) There is a certain amount of variation in the number of pelvic foramina. Asymmetry occurred in one case, C.

The fact that the fin innervation area has a greater extent caudally and rostrally in the embryo than in the adult, offers no contradiction between the rival theories. On the side fold excitation theory

and expect to find a greater rostral extension in the embryo, and greater caudal extension is also explicable on the assumption that contraction of the fin area has also taken place caudally.

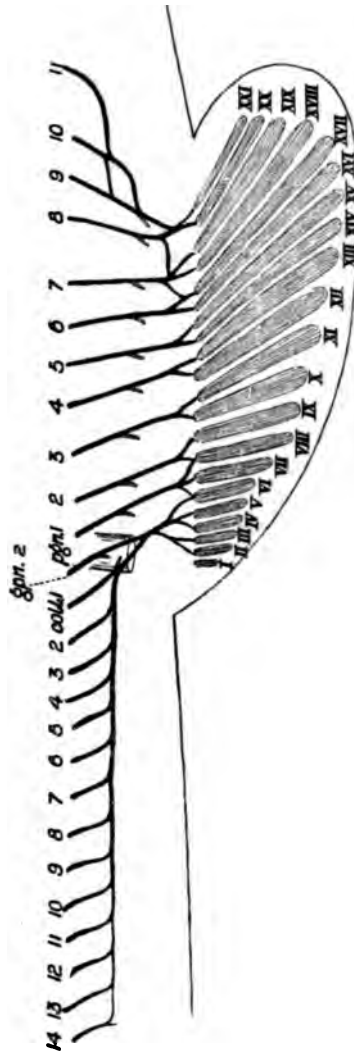


FIG. 7.—Diagrammatic representation of left pelvic fin and nerves in an embryo 31 mm. long. After reconstructions. The dorsal portion of the fin plexus is alone shown. The ventral branches are indicated in paler tint. The fin is supposed to be viewed from the exterior, the greater part of the girdle and the fin skeleton being removed. Fin muscles, parallel shading.

In the migration theory we have already (p. 17) found it necessary to assume that one or two segments just rostral to the collector area in adult are potential collector segments. The greater number of motor nerves in young embryos lends ample support to this view. *gain, since on this theory it is held that the formation of a collector*

is due to migration of the fin, we must explain the presence of the posterior collector in the embryo as being due to a secondary rostral wandering of the fin. In the case of *Mustelus* ((12), p. 348), it was shown that a portion at any rate of the nerves forming the posterior collector gave up this condition later, and ran down to the fin as separate nerves. Now in the two male embryos, E and F, nerves 9, 10, and 11 of the post-girdle nerves form a posterior collector. From Table III we learn that the average number of post-girdle nerves in the male is 9.45. Consequently it is not unlikely that the more rostral of the nerves forming the posterior collector in the embryo will afterwards give up the collector condition and run separately to the fin. Such a state of things would, as in *Mustelus*, be an argument for regarding the collector state as more primitive than the condition in which the nerves run down separately—which latter state is to be regarded as the more primitive on the side-fold theory. Much stress, however, cannot be laid on this owing to the small number of the embryos examined.

Concerning the question of ontogenetic migration of the girdle no answer can be given in the case of *Acanthius vulgaris*. When we take into account the great amount of variation which occurs in the adults, it is obvious that in order to obtain a satisfactory answer it would be necessary to determine the serial number of the nerves piercing the girdle in a large number of embryos, and to compare the mean with that of a large number of adults. Such a course with the present methods of investigation would involve a labour of some years. Consequently I have left it unattempted. In the case of *Mustelus* a certain amount of evidence was collected which tended to show that no such process occurred ((12), p. 348).

Apart from the case of *Torpedo nuxce*, where both Braus and Mollier have agreed in considering that some such process occurs in the pectoral girdle ((11), p. 589 and following), the former has attempted to prove that ontogenetic shifting of the fin occurs in *Spinax niger*.

The evidence, however, is open to criticism since no account is taken of variation. In support of such ontogenetic migration Braus adduces two pieces of evidence: (a) during embryonic development a shifting occurs in the relation of the muscles to the nerves of the fin; e.g., a nerve which is an embryo of 2.6 cm. supplies muscles IX and X comes in an embryo of 3.2 cm. to supply muscles VIII and IX, and so on ((11), p. 568); (b) "die Nervenkanäle der Gliedmassengürtel schliessen während der verschiedenen Phasen der Entwicklung verschiedene serielle Nervenstämme ein" ((11), p. 588). In a table on p. 620 Braus gives diagrams showing the condition of the plexus in adult and in four embryos of different stages after the formation of the fin skeleton. He finds that in embryos of 31.5 and 32.0 mm., the girdle-piercing nerve is 29, in an embryo of 40 mm. it is 30, whilst in the adult again it

is 29. This he takes as evidence that the girdle migrates caudally between the stages of 32·0 and 40·0 mm. and then rostrally again between the stage of 40·0 mm. and the adult ((11), pp. 592, 593). To base such an important deduction on four specimens, whilst completely ignoring the possibility of his 400 mm. stage being merely a variation, seems greatly to weaken the case for ontogenetic migration of the pelvic fin in *Spinax niger*. Braus' argument rests on the assumption that no variations occur in the position of the girdle-piercing nerve in this species. Though I have never had an opportunity of examining *Spinax*, I have been able to make observations on a number of specimens of each of the following species—**Mustelus vulgaris*, *M. lavis*, *Acanthias vulgaris*, *Galeus cunis*, *Carcharias glaucus*, *Scyllium canicula*, and *Scyllium catulus*. In all these species there occurred variations of several metameres in the position of the girdle. Until, therefore, it has been definitely shown that *Spinax* is constant with regard to the position of its pelvic girdle, any deductions based on the assumption that no variations occur in this species are to be accepted with the greatest caution.†

General Remarks.

As has already been stated, the idea with which this investigation was commenced lay in ascertaining the amount of variation shown on certain points in a given species from a given locality, and to discover how far these were explicable on either of the two rival theories—the side-fold theory and the migration theory of the origin of limbs.‡

It has been seen that certain of the facts are not in accord with the side-fold theory, without considerable strain on the imagination, though all are explicable on the theory of migration. Consequently the general tenour of the paper is on the side of limb migration.

* Twenty-three specimens obtained from Plymouth show a range of variation with regard to the girdle-piercing nerve, post-girdle nerves, and whole vertebrae amounting to four metameres. This species is the common form in the Channel. I have never been able to procure *M. lavis*, and according to Day ((2), vol. 3, p. 295) it does not occur here. At Naples, *M. lavis* is the common form, *M. vulgaris* being much scarcer. I have previously given evidence to show that *M. vulgaris* is a more stable form than *M. lavis* in the Mediterranean ((12), p. 342). We may look upon this as evidence for the greater variability of the more abundant form, or possibly we might regard the *M. vulgaris* of the Mediterranean as having acquired greater stability by the splitting off of a variable factor as *M. lavis*. This, of course, is mere speculation.

† Since the above was written I have had the opportunity of examining a number of specimens of *Spinax niger* on this point, and find that here, as in all other Elasmobranchs examined, there exist variations in the position of the pelvic girdle amounting to several metameres.

‡ This paper is not concerned with the more precise origin of the free limb, whether it has been derived from gill-arch rays, according to Gegenbaur, or from external gills, as recently suggested by Graham Kerr ('Proc. Cambridge Phil. Soc.,' 1899).

One fact, however, must be borne in mind. It has been assumed throughout that the vertebra and nerves of any given segment in one specimen are homologous with the vertebra and nerves of the segment with the same serial number in another specimen. In other words, to quote Bateson ((4), p. 32), "it has been assumed that the individuality of each member of the meristic series is respected." How far such a proceeding is justifiable is open to question. All that can be urged in support of it is, that firstly, we have been dealing with members of the same species all through, and secondly, that if the assumption is made, the variations under consideration no longer appear as a mass of disjointed facts, but group themselves with a certain coherence as the results of a process going on in this region of the body, a process of which we do not understand the cause, but to which has been given the term of limb-migration.

The facts recorded in this paper may be summarised as follows:—

(1.) Considerable variation occurs in *Acanthias vulgaris* with regard to—

- (a.) The serial number of the girdle-piercing nerves.
- (b.) The number of the post-girdle nerves.
- (c.) The number of nerves forming the collector.
- (d.) The number and position of the nerve canals.
- (e.) The number of the fin rays.
- (f.) The number of the whole vertebrae.

(2.) Asymmetry occurred in an appreciable number of cases.

(3.) Differences occurred in the two sexes on the following points. The position of the girdle was more rostral in the male than in the female. The post-girdle fin innervation area is greater in the male than in the female, owing to the development of the mixipterygium.

(4.) The female is on the whole more variable than the male.

(5.) A well-marked correlation exists between—

- (a.) The position of the girdle and the number of collector nerves.
- (b.) The position of the girdle and the number of post-girdle nerves.
- (c.) The position of the girdle and the number of whole vertebrae.

(6.) No correlation was found between the number of the fin rays and the number of fin nerves.

(7.) At certain stages in ontogeny the number of collector nerves is greater than in the adult.

(8.) At certain stages in ontogeny the number of post-girdle nerves is greater than in the adult. The most caudal two or three of these form a posterior collector—a structure which is never found in the *hult*

The facts recorded have been used as criteria between the two rival theories of limb origin with the following results :—

(1.) To explain the variations on the side-fold excalation theory, it must be assumed that excalation of segments is going on in the collector and pre-collector areas, whilst, at the same time, intercalation is taking place in the post-girdle area ; or, in other words, that the portion of the vertebral column in front of the girdle is tending to split up into fewer segments, whilst simultaneously that portion behind the girdle is tending to become divided into more segments. Leaving on one side the improbability of two contiguous portions of the vertebral column undergoing at the same time two opposite processes, an examination of the number of whole vertebræ associated with different positions of the girdle lends practically no support to the view that intercalation is going on in this area.

(2.) On the side-fold excalation theory an explanation of the variations in the position and number of the nerve canals of the girdle and of the occasional instances of asymmetry, necessitate the assumption that the pelvic girdle in different specimens is not homologous—an assumption which at present seems unjustifiable.

(3.) The different variations observed are not discordant with the view that the limb is capable of migrating along the body, on which view it must be supposed that a secondary rostral migration has followed a primary caudal one. Moreover, such a view receives confirmation from the existence of a posterior collector, and of a more extensive anterior collector in certain embryonic stages.

BIBLIOGRAPHY.

- (1.) v. Davidoff, M., 'Beiträge zur vergleichenden Anatomie der hinteren Gliedmasse der Fische. Morphol. Jahrb.,' vol. 5, 1879.
- (2.) Day, F., 'The Fishes of Great Britain and Ireland,' 2 vols., London, 1880—84.
- (3.) Paterson, A. M., "The Human Sacrum," 'Proc. Roy. Soc.,' vol. 51, 1892.
- (4.) Bateson, W., 'Materials for the Study of Variation,' London, 1894.
- (5.) Mollier, S., "Die Paarigen Extremitäten der Wirbelthiere, I. Das Ichthyopterygium," 'Anat. Hefte,' vol. 1. 1894.
- (6.) Gadow, H., and Abbott, E. C., "On the Evolution of the Vertebral Column of Fishes," 'Phil. Trans.,' B, vol. 186, 1895.
- (7.) Bumpus, H. C., "A Contribution to the Study of Variation," 'Journ. Morph.,' vol. 12, 1897.
- (8.) Fuerbringer, M., 'Über die spino-occipitalen Nerven der Selachier, u.s.w.,' Leipzig, 1897.
- (9.) Waite, F. C., "Variations in the Brachial and Lumbo-sacral Plexi of *Neoturus maculosus*," 'Bull. Mus. Comp. Zool., Harvard, Cambridge, Mass.,' vol. 31, 1897.
- (10.) Braus, H., "Über die Innervation der paarigen Extremitäten bei Selachiern, u.s.w.," 'Jena. Zeit.,' 1897.

- (11.) Braus, H. "Beiträge zur Entwicklung der Muskulatur und des peripheren Nervensystems der Selachier," 'Morphol. Jahrb.,' 1899.
 (12.) Punnett, E. C., "On the Formation of the Pelvic Plexus, &c., in the Genus *Mustelus*," 'Phil. Trans.,' B, vol. 192, 1900.

"On the Application of Maxwell's Curves to Three-colour Work—
 with Especial Reference to the Nature of the Inks to be
 employed, and to the Determination of the Suitable Light—
 filters." By REGINALD S. CLAY, B.A., D.Sc. Communicated
 by Sir W. DE W. ABNEY, K.C.B., F.R.S. Received April 25 —
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PART I.—THEORETICAL.

1. *Three-colour Projection.*

Maxwell showed that any colour in the spectrum could be matched by a suitable mixture of three monochromatic lights, red, green, and violet, and his celebrated curves give at each part of the spectrum the intensity of these lights necessary to imitate the colour at that point, both in hue and luminosity. Other experimenters have since repeated his measurements with improved apparatus; and throughout this essay I shall employ the curves found by Sir Wm. Abney as being probably the most accurate.†

Thus it is possible to photograph a spectrum in three colours only. Three negatives must be taken through "filters" which allow the colours to pass respectively in amounts determined by the above curves. One filter must allow light to pass according to the red curve, *i.e.*, the extreme red is just transmitted, and the bright red fully passed. Then the filter absorbs the yellow slightly, and the absorption increases along the spectrum, until in the blue and violet it is nearly complete. From this negative a transparency is made, and projected with red (monochromatic) light on a white screen. So for the green and violet. Assuming this done, we have a spectrum illuminated everywhere with the three monochromatic lights in the proportions indicated by the curves. Thus the spectrum will be reproduced exactly. (See Notes 1 and 2.)

If, instead of illuminating the three transparencies with monochromatic lights, we use red, green, and violet lights produced by passing white light through coloured glasses (providing that these lights are of the same dominant hue as the primary colour sensations), we shall still obtain a spectrum that appears almost correct. The light obtained by a coloured glass is not monochromatic, but, if the hue is

† "The Colour Sensations in Terms of Luminosity," 'Phil. Trans.,' 1899.

correct, it may be matched by a mixture of the monochromatic light and white. Thus the colours on the screen will not be true spectrum colours, but spectrum colours diluted with white.

The amount of white, of course, depends upon the glasses used, and in matching some parts of the spectrum there will be more white than in others. Yet the result is very good; indeed, without direct comparison the white would hardly be noticed. The beautiful pictures produced and exhibited with such coloured glasses by Mr. Ives is a very good proof of the fact that an admixture of white is of very small consequence.

Note 1.—The above is not strictly accurate, as it is not possible to match some parts of the spectrum even when using pure spectrum colours. The match can only be made after white has been added. The reason for this can be seen from the curves. Owing to the overlapping of the sensation curves, the green sensation is nowhere excited alone, but is always accompanied by a small excitation of the red and violet. Thus the spectrum green—although really a pure colour—excites all three sets of sensations in the eye, i.e., it produces the sensation green and white. Now, to match the yellow, we require to excite only green and red, and this we cannot do using spectrum red and green, for the green always excites to some extent the violet sensation. This violet combines with proper proportions of green and red to produce white, so that we can imitate the yellow when it is mixed with a little white, but not the pure yellow alone. The same is true of the other end of the spectrum where the red sensation should only be slightly excited.

We shall in what follows neglect this necessity for the addition of white in speaking of colour matches as it only complicates the question without materially affecting the results. (Where my statements require modification in consequence I shall indicate it by a star (*).)

Note 2.—In Abney's paper above referred to, he shows that the third sensation is probably not violet, but a blue, which is near the blue lithium line in the spectrum. As I shall be using the word "blue" in speaking of the double colour blue ink, to avoid confusion, I shall throughout refer to the third colour sensation as "violet." I shall, however, use Abney's curves, and by the term "violet" shall mean his "blue sensation."

2. Three-colour Printing.

When we turn to printing, matters are not so simple. One's first impulse would be to select those parts of the original picture in which red light occurs, or which excite the red sensation, and print them with a red ink, and so with the green and violet; but a little consideration will show that *this would not be right*. For instance, a yellow object affects both our red and green sensations, according to Maxwell's

curves, and thus we should have to print it with the red and green inks. Now the original yellow was equivalent to the addition of a red and green light, so that it ought to be brighter when both colours are there than when only one. The reverse would be the case if printed as above, and, of course, as a white object reflects all the colours, it would be printed with all three inks. Now, though red, green, and violet lights when added give white, red, green, and violet inks superposed on white paper certainly will not make white.

In fact, inks produce colour by absorption. Thus, instead of successive inks adding to the light, they each reduce it.

Suppose, then, we proceed the other way, and instead of looking for those parts of the picture where red is reflected and printing with red ink, we print those parts where no red is reflected with an ink which absorbs red, but reflects all the other colours. This will be a bright bluish-green ink. In the same way we will print those parts which reflect no green with an ink which absorbs that colour; this will be a bright magenta or almost a pink. And those parts which reflect no violet we will print with a yellow ink. Now a yellow object reflects red and green but no violet. Hence it will have to be printed with the violet absorption ink only—that is, the yellow—and with neither of the others. A red object reflects neither green nor violet, and will be printed with the pink and yellow inks, which will leave red only. As a yellow object is printed with only the one ink, but a red one with two inks, the yellow will be the brighter, and this is as it should be.

As the inks are to absorb red, green, and violet respectively, they will, roughly speaking, be complementary to those colours, that is, when added to them either by a double-image prism, or by reflection from a clear glass surface, they should give white. Still, complementary is a very vague description of a colour, and it must be our aim presently to define it more precisely.

The above theory is due to Mr. Ives.

3. *Application of Maxwell's Curves.*

As we said at first, these curves were derived by adding three *monochromatic* lights, and are only strictly true of such colours. If, however, we use lights compounded of—or, at least, capable of being matched by—one of these monochromatic colours and white, we shall still obtain a result very nearly as perfect as with the pure colours. It can easily be seen, however, that if any other colour than white were combined with the pure colours the result would be spoilt:

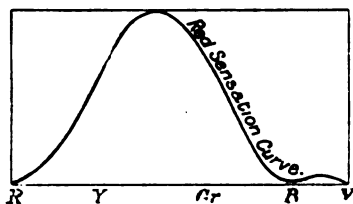
All colours, including white, can be matched both in hue and luminosity* by combining the three monochromatic colours, red, green, and violet, in proportions given by Maxwell's curves. It is easy to

* See Note 1, p. 27.

reproduce them by adding lights : is it possible to do so by printing with inks ? It is, at least, theoretically. The result, however, would be a very dark picture of no use in practice, as it would require to be looked at either in full sunlight or some equally powerful light.

Suppose, then, we take a card and first print it all over with an ink that absorbs all the spectrum except a narrow band in the red, another narrow band in the green, and a third in the violet (whether such an ink could be found is immaterial as our aim is purely theoretical). If the width and position of these bands are correct, *i.e.*, if the colours left match the three sensation colours in hue, and their luminosities are properly proportioned, the card will now appear a neutral grey. In a powerful light it will appear white. This is to be our white.

The inks must each absorb one of these bands of colour and leave the other two bands unaffected. The yellow ink will absorb the violet, the pink ink the green, and the blue ink the red band.



Now if we print with these inks in amounts which are the complementaries of the amounts indicated by Maxwell's curves, we shall be transmitting lights in amounts which are the same as those given by Maxwell's curves, and thus we shall exactly match all the colours.† A picture so obtained will be correct both in hue and luminosity,* but it will have to be examined in a very powerful light.

The three bands of colour left, referred to above, ought all to be very narrow so that the light may be approximately monochromatic.

But now suppose these bands not to be infinitely narrow. The light will no longer be monochromatic and a single band will excite more than one colour-sensation in our eyes. For instance, a band in the red—unless in the very extreme red—excites both the red and green sensations ; a band in the green excites all three (the red and violet will be nearly in the proportion to form white, and, if desired, the colour may be made equivalent to green and white by a proper selection of the width and position of the band) ; a band in the violet—unless in the extreme violet—will excite also red and green

† The curves referred to above are those in which equal heights of ordinate form white, *e.g.*, Abney, *loc. cit.*, p. 284, fig. 8.

* See Note 1, p. 27.

with generally the green in excess of that required to combine with the red to form white. Thus with such bands for our primary colours, we should have not the true sensation colours, but those colours with an addition of either green or white. The white will not matter much, but the green will, of course, spoil the effect.

With these bands more light is used, and both the grey produced by the original printing and the final colours will be much brighter ; thus the picture could be examined in a less powerful light.

Lastly, imagine the bands still wider ; in fact, allow them just to touch, so that they divide up the whole spectrum between them ; and suppose each ink to exactly absorb one of these bands. The first printing in neutral colour is no longer necessary, and we shall see the full white of the card. Also the picture will no longer require a strong illumination. But we shall now have our primary colours mixed with large amounts of other colours. The red will be mixed with green, the violet will be diluted with green and red (see Abney's curve, *loc. cit.*, p. 283), and the green with both red and green in proportions which may or may not form white.

Note.—It is possible to arrange that the red and violet in the middle band shall make white, but it is not possible to so arrange that the red band shall be diluted with white, for there is no violet at the red end of the spectrum. Thus the blue ink cannot be made the complementary of the red sensation if it is to leave a red band of any reasonable width, unless it has an absorption at the violet end of the spectrum, and this introduces a very serious fault, as the same colour would then be absorbed by more than one ink. (See 6.)

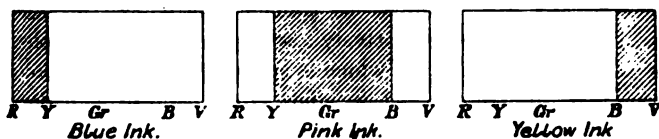
It will now be obvious that if we print these inks in amounts the complementaries of the Maxwell's curves (as in the theoretical case above) the colours we produce will be far from correct. The red will be diluted with green and so on.

If the red could be diluted with white instead of green the picture would be much improved ; in fact, it would be almost as good as if the colours were pure. And fortunately this is quite possible, not however by modifying the inks but by altering the proportions in which they are printed, *i.e.*, by varying the curves to suit them.

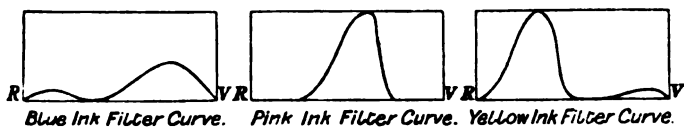
Suppose, for instance, we want to print a colour to match the red sensation. If we print with the green and the violet absorption inks—the pink and yellow ones—we shall leave the red end of the spectrum, which the curves show to excite the red and green sensations. We want the red only, or—if we cannot obtain that— we must be content with red and *white*. This we can obtain with our inks by printing the violet absorption ink—the yellow—rather less fully. The violet and green then, with a part of the red will form white, and leave red only. So with any colour, if we cannot obtain it pure, we *always match it when diluted with white*. The amount of white

so formed can be increased in any part of the spectrum by printing all three inks less fully, and the best result will be obtained when the added white is everywhere proportional through the spectrum to the luminosity of the colour.

The three inks we have been discussing are supposed to absorb the spectrum thus :



The curves to be used in printing these must be adjusted so that the spectrum may be matched in luminosity as well as hue, the added white being itself (roughly) proportional to the luminosity. To adjust the luminosity of the colours, it will be observed that the colours which are relatively too bright can be reduced by printing more fully with all three inks. The curves are roughly



I have calculated the exact curves for such inks to enable them to match exactly the colour sensations as determined by Abney, and the results are given in the second part of this paper.

4. The Effect of the Addition of White.

There are several ways in which this can be shown not to be important when the amount of white is not very great.

In the first place, we are accustomed to use colours which are diluted with white. According to Abney the light reflected from vermilion contains 2.5 per cent. of white, from cobalt 55 per cent., from French ultramarine blue 61 per cent., and from chrome yellow 26 per cent. of white (p. 166, 'Colour Measurement'); whilst in the same way the light transmitted by a ruby glass contains 2 per cent. of white, by a canary glass 26 per cent. of white, by a green glass 31 to 61 per cent., by a cobalt glass 42 per cent. of white. That is to say, the transmitted light from a cobalt glass can be matched by 58 per cent. of a spectrum colour with 42 per cent. of white.

Then again about 3 per cent. of white can be added to orange, rather less to green, and so less along the spectrum till 0.8 per cent. in

the blue-green can be added *without being perceived* even when the diluted colour is compared directly with the pure (p. 132, 'Colour Measurement'). It can thus easily be believed that much more can be added without appreciable effect when no direct comparison is made with the pure colours.

But probably the best example of the unimportance of the addition of white is given by Mr. Ives's results, where the colours used as primary colours are produced by transmitting white through a red green, and violet glass respectively; when, as in the examples above there is transmitted a light which is equivalent to a pure colour with a percentage of white, which in some cases is quite considerable. I confess my surprise at the excellence of the results obtained, which show how exceedingly bad a judge the eye must be of an addition of white to a colour. Provided the quantity that will be left in the printing is not greater than the quantities he obtained by the transmission through his glasses—and I see no reason why inks should not be produced to secure this—the pictures obtainable by the three-colour processes ought not to be inferior to those he obtains by the superposition of the three-coloured transparencies.

We can realise the effect of this addition of white by supposing a coloured picture projected on a screen in a room that is not quite dark. The screen will then reflect, in addition to the colours projected a certain amount of the diffused light of the room. As this coloured picture would not consist of pure colours undiluted with white, the addition of further white will make the proportion greater than would occur in printing.

On the other hand, the eye is a very good judge of *hue*, a very small variation in the proportion of the colours (other than white) being easily detected.

Much may be done, by training, to educate the eye to appreciate the relative luminosity of the colour, or, as it is usually termed, the "value" of the colour, and many artists are able to recognise variations in this almost as easily as in the hue itself. But the average person is not a good judge of this, and it is of much less importance than the hue. For instance, many people fail to see any improvement in a photograph of a landscape or a bunch of flowers taken through an orthochromatic screen, over a photograph of the same subject taken in the usual way.

But circumstances combine to make it difficult to recognise the addition of white even in a picture: the irregularities in the varnishing and the dust upon it are bound to add a proportion of white to the light reflected, and indeed it is very seldom that a picture is so hung that the varnish does not reflect a large amount of white, which we often do not notice until we attempt to take a photograph of it, and *then very special lighting* is found absolutely necessary.

5. Printing with Black.

As we shall obtain all our colours diluted with white, one is tempted to ask whether printing with a black ink will not enable us to get them pure ; that is, as we can copy any colour, except for the addition of white, by the three printings in yellow, blue, and pink inks, could we not, by the addition of black, match the colours exactly ?

Suppose it is the red we are matching. We shall get a red with a certain percentage of white. For instance, for 100 parts of red light reflected there may be 10 green and 10 violet. If we now print 10 per cent. black what will be the result ? If the black is a good one, *i.e.*, if it absorbs uniformly all along the spectrum, we shall now have reflected 90 parts red, 9 green, and 9 violet. In other words, we merely darken the whole, without in the least altering the proportion of white. The addition of black will then not improve this purity of the colours ; it will only make them "dirty."

6. Production of Colour by successive Absorption of Light.

There is a very fundamental difference between this and the production of colour by successive addition, as in Ives's triple projection. In the latter case, if any spectrum colour is thrown on the screen by one light—say, the green—and the same spectrum colour is transmitted also by a second light—red, for instance—the amount of that particular colour is doubled, and the result is an arithmetical addition of luminosity.

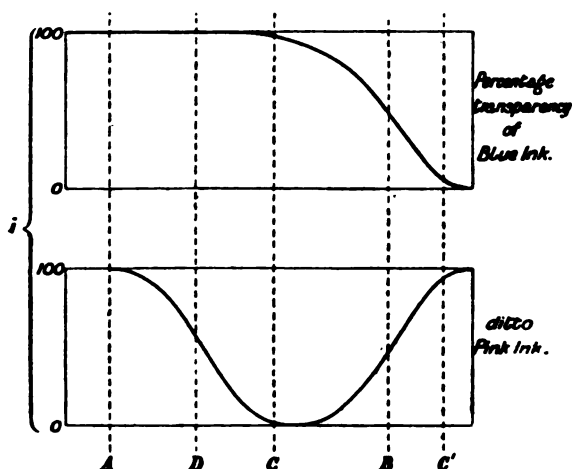
But in printing, if a spectrum colour is completely absorbed by one ink—say, the pink—and also absorbed by another—perhaps the blue—the total light of that colour absorbed by the superposition of the inks is not twice that removed by a single one. The absorptions do not successively subtract light. If two inks each would separately transmit one-tenth of the light of a given wave-length, the two inks together would transmit not one-twentieth, but only one-hundredth, of the light of that colour. The law is a geometrical and not an arithmetical one.

In "process" three-colour printing this is of the utmost importance, for there the inks are always printed full strength, and the tint is regulated by the size of the dots, that is, by the percentage area of the paper which is covered by the ink. The dots are produced by placing a ruled screen in front of the negative while it is being exposed, and the dots at any part, when developed and etched, have areas very nearly proportional to the intensity of the light which fell upon that part. Each colour is produced by similar dots, and when the three inks are printed these dots partly overlap one another. As the ruling is *very fine and the dots are very closely spaced*, it is impossible in

ordinary commercial printing to control the extent to which they overlap; that is to say, the successive impressions will, owing to minute differences in "registering" the paper, have the dots of the different colours more or less displaced in relation to each other.

If, for instance, the blue dots and the pink dots happened each to exactly cover half the area, in one impression they might be exactly superimposed, and in another they might hardly overlap at all. In the former case half the area will be printed with both pink and blue, and half will be white. In the latter case half the area is printed with blue and half with pink, and there is no white left. If these impressions are to be equally good, the resulting absorptions through the spectrum should be the same in the two cases.

Let the adjoining curves represent the percentage transparency of the inks. At A both inks are perfectly transparent, and as the colour is not absorbed by either, it can of course make no difference to this colour if the dots are superimposed or not.



At B each colour absorbs half the light and reflects half the light. When the dots are printed adjacent to one another, half the total light of that colour will be reflected. When they coincide, half the paper is



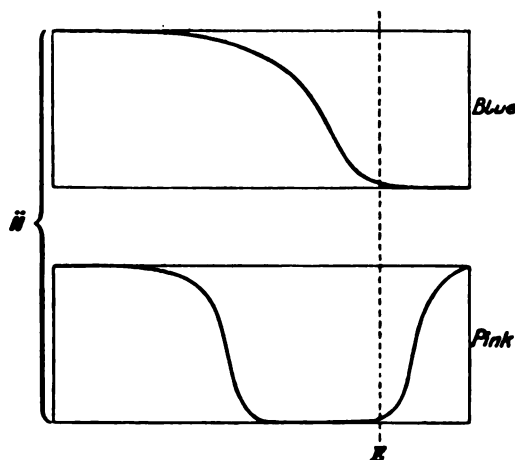
left white: this reflects all the light it receives, which is half the total. The blue ink on the other half absorbs half the light which it

receives of this colour (B), and the pink absorbs half of what is left, so that finally a quarter of the light falling on that half of the paper—or one-eighth of the total light of this colour—is reflected. Adding this to the white reflected from the white half, we see that *five-eighths* of the total light of this colour is now reflected, instead of the *one-half* which is reflected when they are adjacent.

The colour at C is entirely absorbed by one ink and is unaffected by the other. The pink ink absorbs the light which falls on its half, and as the blue ink does not affect this colour it will not matter whether it is on the pink or adjacent to it.

Also at D, where one ink is partly transparent and the other completely so, it cannot matter whether the perfectly transparent one is above or adjacent to the other.

If one or other of the inks is perfectly transparent at every point, the extent to which the dots overlap is immaterial. If the absorptions of the inks



overlap one another, as in Diagram 2 at E, the results are much worse. This colour is entirely absorbed by each ink. If then the dots are adjacent, the colour is absorbed everywhere and none reflected. But when they coincide half the paper is white, and therefore half the total light of that colour is reflected. Thus the reflected light of that part of the spectrum where the absorptions overlap will vary from nothing up to half the total light.

In the case of the pink and blue inks this overlapping would occur at the yellow, where their absorption terminates, the very brightest part of the spectrum. According to Abney's curves, the luminosity of the yellow from λ 56 to λ 60 is about 50 per cent. of the whole white light.

Thus the addition of one-half of this band of light would be sufficient

to entirely spoil the result, and this addition would be produced by a displacement of the dots of about 1/300th of an inch.

It is obvious, then, that in process work the absorptions of the inks *must not overlap*. On the other hand, if we are to be able to produce a black, the absorptions *must meet*. Thus in process work (in the case of the blue and pink inks at least) the absorptions must *just* meet without overlapping, and therefore they *must end abruptly*. It will not matter so much in the case of the yellow and pink inks, for the part of the spectrum where their absorptions meet is of far inferior luminosity.

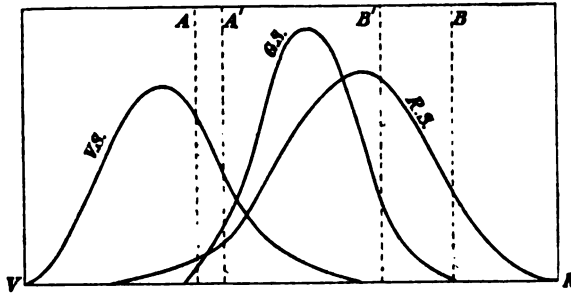
In collotype printing this difficulty would not arise in the same way. Here the tint is produced by the strength of the colour, the lighter shades being produced by covering the whole surface with a thin layer of ink. In this case there would be no uncertainty due to imperfect registering. But at the same time the ink curves for a thick layer of ink are very different to those for a thin layer, as is shown by the measurements I have added of the curves of different depths of the same ink. Thus the proportions in which the three inks should be combined to match a certain spectrum colour will vary with the luminosity of that colour.

To illustrate this. Suppose an object all one colour, such as a cast, to be illuminated by a monochromatic light such as a sodium flame, and a photograph taken, the result ought to be a picture in shades of yellow. But if the curves of the same ink when printed to different depths are different so that the proportions of the colour sensations that it reflects are different, the proportion in which it is combined with the other two inks to match a bright yellow will not be the same as that required to match a dull yellow of the same wave-length. Thus if the filters are adjusted to give the exposures on the three plates that will reproduce that hue in, say, the high lights, it will not be correct in the half-tones, and will be still worse in the shadows. The ideal ink for this process would be one which would absorb the colours in the same relative proportions, whatever the depth to which it was printed. The curve would be one of perfect transparency up to the absorption band, then a sudden drop (for the full colour), say, to about 2 per cent., to remain at this height till the end. The curve should have no rounded corners.

It will be seen that the sharpness of the drop depends on the depth to which the colour is printed; thus for "process" work, where it is always printed to the same strength, the absorption can be made abrupt by using a very full tint. But if this is to be done, the ink must be a very transparent one to the part of the spectrum it is not supposed to absorb, for, as we have seen, it is very important that the same part of the spectrum should not be absorbed by more than one colour.

7. *Best Limits for the Ink Absorptions.*

Assuming now that the absorptions should be abrupt, the next question is, what limits should they have to give the best effects? If one is at A and the other at B in the accompanying diagram—in which equal heights of each colour would give white—the yellow and



blue inks will be complementary to the colour sensations, but they will be very pale, for they reflect large quantities of all three colours. The pink ink will be very dark, it will reflect only a very small amount of the red sensation, and as the violet reflected is not equal to the red, it will not be complementary to the green sensation. It could be made so by shifting A further to the violet, so as to reduce the violet reflected by the pink ink—in fact, until the areas of the red and violet are equal. It is obvious that this would give absurd inks; the yellow and blue would be so pale as to be mere tints, and the pink so dark as to appear purple.

If the absorptions are supposed to end at A' and B', the spectrum will be much more evenly divided between the three colours, each will be slightly pale, but now *neither* will be the complementary of the colour sensation. For instance, if the yellow ink is to be complementary to the violet sensation, it should absorb either violet or violet and equal amounts of red and green, measured by equal areas on the above curves. But at A' (or at any other line between A and B) the green sensation is evidently in excess. In the same way if the absorption of the blue ink extends from the red end of the spectrum beyond B, green is absorbed as well as red, and the ink cannot be complementary to the red sensation. To make this ink complementary, it must have another absorption band somewhere in the violet, and the difficulties we have been considering above will be introduced, for there will then be a part of the spectrum absorbed by more than one ink, namely, by the yellow and the blue, and in places where these inks are printed side by side the absorption of this colour will be twice as great as in those where they are on top of one another.

Thus the blue ink should *not be the complementary* of the red sensation. And in fact the ink will be far from complementary to the red. I find by taking the areas of Abney's curves up to B' with a planimeter, that it should very nearly match the green at about $\lambda 50.3$.

The yellow ink would be made complementary to the violet sensation by ending the absorption at A, but it would be very pale, for it would transmit a large amount of white, as there is a large proportion of the violet beyond A. The result will be an unnecessary amount of white in the final picture. The result will be better if the absorption reaches to A'. Thus this ink also *should not be complementary* to the violet sensation. The pink ink at the red end of the spectrum transmits red and green, and at the other end chiefly violet and green. If the violet is equal to the red it will be complementary to the green sensation. This can be achieved by moving A towards the violet end, but only at the expense of the yellow, which would be rendered very pale. So this ink also *should not be complementary*.

I have gone into this rather fully as it has so frequently been stated that the inks ought to be complementary to the colour sensations. In a very rough and general way this is true, but there is no advantage in such inks; on the contrary, they would, even if obtainable with abrupt absorptions, not be so good as others that are not complementary. The only possible advantage in using complementary inks would arise in the case in which the absorptions were abrupt, for then the filters would be complementary to Maxwell's curves. But the advantage here is only one of theory, the printed result being, as we have seen on the preceding page, a picture with a very large excess of white. As in practice the filters would be adjusted by trial, even this advantage is illusory.

To summarise. The inks could only be complementary—

1. If they had abrupt absorptions with limits near the ends of the spectrum, when the colours would be in some cases mere tints; or
2. If they have overlapping absorptions, or if some colours are absorbed by more than one ink, both of which are bad, since the resultant absorption will follow a geometrical and not an arithmetical law.

The exact positions of the limits will, no doubt, finally depend on the fact that there will be very few pigments which sufficiently fulfil the conditions, and the blue and pink ones with abrupt absorptions near the yellow will not be numerous. Still it is theoretically interesting to determine the best positions, apart from the difficulty of finding inks to suit. This will be best done by calculating the curves for different absorption limits and estimating the amount and distribu-

tion of the added white, and the luminosity of the resultant colours in each case. I have worked out the curves for two interesting positions of the absorption limits.

8. Choice of Inks.

The mere appearance of an ink, even when a rubbing on paper is taken, is not much guide, as the eye is not suited for selecting the inks by their hue; for they are double-colour inks, and we know that the eye cannot at all judge of the composition of a yellow or a blue. Yellow, for instance, might be spectrum yellow, or a mixture of any part of the red end of the spectrum with almost any part of the green. In other words, the eye cannot distinguish between yellows of a great variety of compositions. But the eye is a fairly good judge of the three primary colours—at least, of their hue—so that if the inks be printed in pairs a fair estimate can be made. The violet produced by the pink and blue inks should be almost pure, containing only a small percentage of white, as the red and green are almost equal. The green should very closely match the green sensation. But the red will contain some green, and should about match the hue of λ 67·4.

But the only true test is the spectroscopic one. Examined spectroscopically—

1. The inks should each absorb one band of the spectrum in the red, green, and violet respectively.
2. Must transmit the remainder unaffected.
3. Must absorb the whole between them.
4. For half-tone work the absorptions must end abruptly, especially in the case of the pink and blue inks.
5. The limits of the absorptions should be so chosen that the added white may be distributed through the spectrum as nearly as possible proportionally to the luminosity of the spectrum, and that
6. The luminosity of the resulting colours in matching the spectrum should be as high as possible.
7. The inks will not be complementary to the colour sensations.

For (3) the absorptions must meet, for (2) they must not overlap; thus (2) and (3) lead to (4).

The above conditions are, I believe, sufficient. I have examined a large number of inks supplied by different firms to see how far they are realised by those at present in use.

As the orange, yellow, green, and blue-green parts of the spectrum are far brighter than the rest, it is here that any conditions laid down must be specially observed. In the blue and violet and in the deeper red it is not so important.

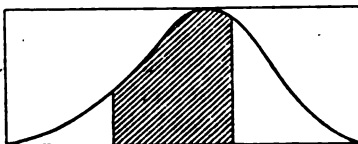
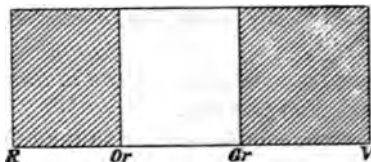
Thus, if the pink ink is only partly transparent in the blue and not an abrupt absorption there it will not much matter, provided perfectly transparent in the red and orange, and has an absorption commencing suddenly in the yellow. Such an ink is Flen "Theoretical Red," No. 1303.

The yellow must be perfectly transparent in the red, orange, yellow and green with an absorption commencing in the blue; but it not matter if it is not very sudden.

The blue ink must be perfectly transparent in the green, blue-green and blue; there must be an abrupt absorption in the yellow. violet does not matter so much. So far I have found no ink to these conditions. All the ordinary inks are too opaque in the red and blue-green.

9. *The Luminosity as well as the Hue of the Spectrum must be Made everywhere.*

It must not be forgotten that most natural colours are composed of a large range of spectrum colour, and the resultant hue depends on the proportion in which these colours are composed. Now although the eye is a bad judge of the luminosity of the components it is a very good judge of the resultant hue of a compound colour (and thus indirectly it is of course able to compare the relative luminosity of its components as far as this luminosity is distinct from white). It follows that the component colours must be rendered in their correct proportions. When a negative is exposed through its light-filter the light that will reach it from a natural object will be the sum of all the colours that the object reflects each rendered according to the curve of the filter. Suppose for instance that all the light from the orange to the green is reflected by the object as in the first figure.



Then if the second figure represents the curve of the light-filter, the area of the shaded portion gives the amount of transmitted

and (supposing for the moment the plate to be equally sensitive to all colours) will be proportional to the final opacity of the plate. If the filter curves have been arranged so as to match the spectrum in luminosity as well as hue, then the resulting print will reflect the sum of light which is equivalent in ray composition to the sum of this band of spectrum colour, and will therefore match it in dominant hue. But if the spectrum has been matched in hue only, the resultant colour will be the sum of the colours in this band mixed in the wrong proportions, and will not have the same dominant hue as the original.

10. *The Light-filters.*

If the inks are neither perfectly transparent or perfectly opaque to each colour, so that there are no parts of the spectrum to which they are only partially transparent, and if also the absorptions do not overlap, the resultant colour will be that left after successive subtraction by the three inks, and will be the same as would result from the successive addition of the three complementary lights in triple projection; and then the filters can be found by determining the dominant hue of the complementary colour.†

The curves for the filters can also be calculated in this case by finding what percentage area the inks should cover so as to transmit light whose composition, in sums of the colour sensations, is everywhere that given by Maxwell's curves. This is how I calculated the curves for the hypothetical inks.

But when we deal with practical inks these conditions are far from being fulfilled, and the filters cannot be found by dealing with the complementary lights. For, as we have seen, since the same colour is absorbed, at least to some extent, by more than one ink, the arithmetical law of absorption will not hold, and the colour resulting from the printing with the inks is not that produced by the combination of the complementary lights. So that in practice we shall have to adopt some other method, *which must be experimental, and must be founded upon impressions of the actual inks.*

11. *Depth of Colour and Process Printing.*

One of the greatest practical difficulties in printing with only three colours, and especially with such strong colours as red and blue, is to ensure that the quantity of ink shall not vary from impression to impression. Usually, very slight variation in the amount of ink is accompanied by a change in the whole tone of the picture. This is because with most inks the curve of absorption is different for different depths of colour, and thus the proportion of the three fundamental

† Abney, '*Photographic Journal*,' January, 1900, p. 121.

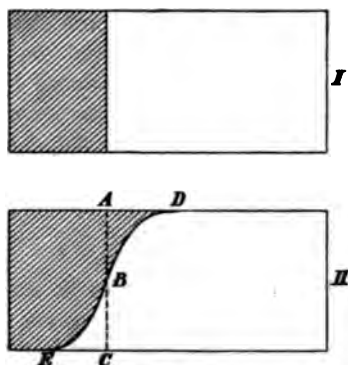
colours left in the reflected light also will vary with the de will not matter much when a large number of colours—fourteen—are used, as the average amount of red and blue the successive inks will be nearly constant; but with only each colour the effect is very marked.

This effect is greatest when the curve for the ink is a g for as the quantity of ink is increased in an arithmetical p the absorption of any given colour will increase in a geom progression, assuming, of course, that the ink is transparent. T the distributing apparatus of the machine is very perfect inc be impossible with such an ink to obtain uniform impression

Were the ink one with abrupt absorption, that is, one very transparent up to a certain colour, and then nearly op variations in the quantity of ink would have very little effe of the greatest difficulties would at once disappear.

12. *Effect on the Purity of the Colour of an Abrupt Absor*

It might perhaps be supposed that inks which reflected m the parts of the spectrum for which they are to be trans gradually reflect less up to the part they are to absorbt nearer approximations to monochromatic inks than those wh tions begin abruptly. That this is not the case can be easily compare the two red-absorption inks opposite. In the seco replaced the reflected light ABD by BEC. Now Maxw show that the proportion of red to green in BEC is much g it is in ABD, so that the absorption of the red is not so comp



it is in I, while the transparency to green is less. Thus II w so good an ink as I. When, for instance, this and the violet ink are printed, which ought to leave green only, there wo red left, and this in greater amount than with the ink I.

mean that to match green more violet would have to be left to form white with the excess red, and the dilution of the green with white would be greater.

13. *Summary of Reasons for using Inks with Abrupt Absorption.*

- i. The colour will not be affected by slight variations of register.
- ii. Variations of the amount of ink in the several impressions will not be so important.
- iii. The colours will be much purer, being mixed with less white.
- iv. The curves for the filters can be found if desired by determining the complementary light, and working as Abney has described in his paper in the 'Photographic Journal' already mentioned.

This last will, however, only be possible if the curves are nearly 100 per cent. up to the absorption in each case, and so far I have found no set of inks, nor even a single ink for which this is the case.

PART II.

14. *Calculation of the Curves for Theoretical Inks.*

By theoretical inks I merely mean inks which are perfectly transparent until the absorption commences, and then are perfectly opaque. With such inks we can calculate the percentage area to be covered to match any given colour by considering the absorptions they produce.

I drew on a large scale the curves found by Abney for the ray composition of the normal spectrum, in which equal ordinates form white. I drew vertical lines where the ink absorptions should terminate, and with a planimeter I found the areas of the curves up to these lines. Thus I obtained the ray composition of the inks.

Thus, taking the vertical lines at wave-lengths 49 and 59, I found the following areas. From the violet end of the spectrum up to λ 49—

Violet.....	122·4	or	Violet.....	119·4
Green.....	4·8		Green.....	1·8
Red	3·0		Red	0·0
			White	3·0

This will be the part of the spectrum absorbed by the yellow ink. From λ 49 to λ 59 the areas were—

Violet.....	25·8	or	Violet.....	0·0
Green.....	128·0		Green.....	102·2
Red	79·5		Red	53·7
			White	25·8

This will be the part of the spectrum absorbed by the pink
From λ 59 to the end of the spectrum they were—

Violet, 0. Green, 15·6. Red, 65·8.

These add up to—

Violet, 148·2. Green, 148·4. Red, 148·3.

Thus the ray compositions of the inks are—

	Yellow ink.	Blue ink.	Pink ink.
Violet	25·8	148·2	122·4
Green	143·6	132·8	20·4
Red	145·3	82·5	68·8

The blue ink matches λ 50·4 and white, and the yellow will m
 λ 58 with white.

I now took the ray composition of some spectrum colour, and f
by successive approximation the area to be covered by the inks to m
it. Thus λ 44 has a ray composition of—

Red.....	1·47	equivalent to	Red.....	1·27
Green.....	0·20	„	Green.....	0·0
Violet.....	76·68	„	Violet.....	76·48
			White.....	0·2

Then if 66 per cent. of the area is free from yellow ink, 66
cent. of the light the yellow would absorb will be transmi
namely—

Red, 0. Green, 1·2. Violet, 76·6. White, 2.

So if 5 per cent. of the area is left free from blue ink it will r
in addition—

Red, 3·2. Green, 0·78.

The pink ink must be printed all over. Then we shall
left—

Red.....	3·2	equivalent to	Red.....	1·22
Green.....	1·98	„	Green.....	0·0
Violet.....	78·6	„	Violet.....	76·46
White.....	2·0	„	White.....	3·98

which matches the spectrum colour except for an excess of
white.

In the same way I worked out the percentage areas to be left
from colour to match each of the wave-lengths 38, 40, 42, &c., 1
70, and have found in each case the excess of white.

I selected the above positions partly because they divide the spectrum about evenly, but also because most of the pink inks have their absorptions at about λ 59, and the yellows at about λ 49. As the blue inks are all bad, it is no use considering where their absorptions are.

Another interesting place for the absorptions to end is at the points of intersection of the ray composition curves, namely, at λ 50.5, where the green and violet curves cross, and at λ 57.5, where the green and red cross. It is useful to compare the excess of white and its distribution in the two cases.

The results of these calculations are given in the following tables:—

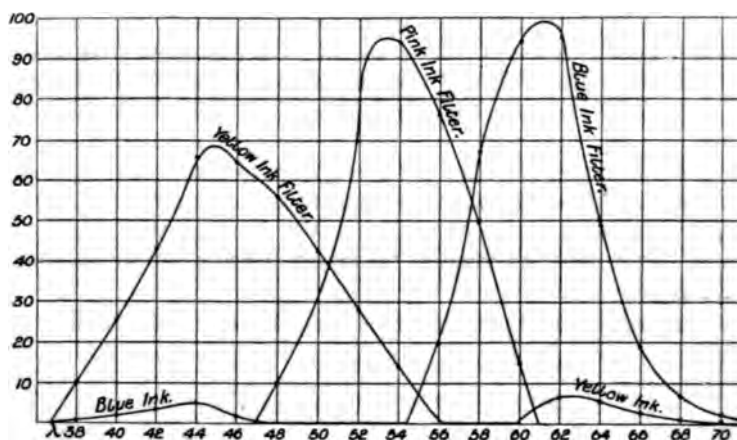
Absorptions at λ 49 and λ 59.

Wave-length.	Luminosity.	Percentage area not covered by inks.			Excess of white.
		Yellow.	Pink.	Blue.	
38	0.25	10.4	—	0.8	0.63
40	0.75	25.5	—	2.0	1.5
42	1.3	43.4	—	3.18	2.6
44	2.1	66.0	—	5.0	3.8
46	3.5	64.0	—	1.4	0.9
48	7.5	56.0	10.0	—	6.7
50	18.0	42.4	31.4	—	19.2
52	56.0	28.0	83.2	—	23.4
54	80.0	14.3	94.4	—	35.5
56	95.0	—	76.0	19.7	16.7
58	99.0	—	50.0	66.5	12.2
60	85.0	—	16.0	94.5	4.1
62	59.0	6.2	—	95.5	0.2
64	26.0	5.3	—	48.4	0.2
66	10.0	2.35	—	19.3	0.07
68	3.2	0.92	—	6.4	0.03
70	0.9	0.3	—	1.8	0.01
					125.74

The sum of the ray compositions for the same wave-lengths is 1120. Thus the excess of white is about 9 per cent.

It will be seen that the excess is chiefly in the brighter part of the spectrum, though it seems rather in the green than the yellow.

These calculations entirely agree with the arguments advanced in the earlier part of this paper to show that the light filters for three-colour printing should be entirely different from those used for three-colour projection.



No. 27.—Filter curves for inks with abrupt absorptions at λ 49 and λ 59. The ordinates show the percentage area to be left free from ink, and therefore indicate the opacity to be obtained on the corresponding negative, or—for orthochromatic plate—the transparency of the filter.

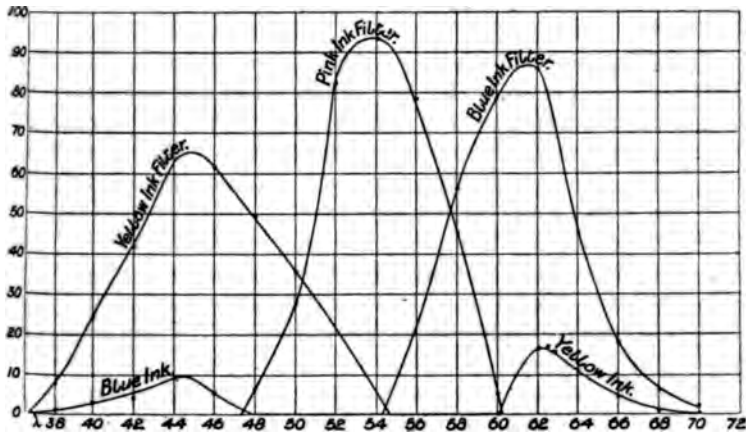
Absorptions at 57.5 and 50.5.

Wave-length.	Percentage area left uncovered by ink.			Excess of white.
	Yellow.	Pink.	Blue.	
38	8.5	—	1.1	1.1
40	24.6	—	3.3	3.6
42	42.2	—	5.9	6.4
44	63.7	—	9.0	9.6
46	61.6	—	5.5	6.0
48	49.3	6.3	—	2.2
50	35.7	28.0	—	6.8
52	19.9	82.8	—	23.4
54	5.3	93.8	—	10.7
56	—	78.5	21.7	7.0
58	—	45.8	57.0	5.2
60	—	5.1	79.0	0.6
62	16.5	—	86.0	22.2
64	11.2	—	46.0	15.2
66	4.2	—	18.0	5.9
68	0.6	—	5.9	1.9
70	0.2	—	2.0	0.6
				128.4

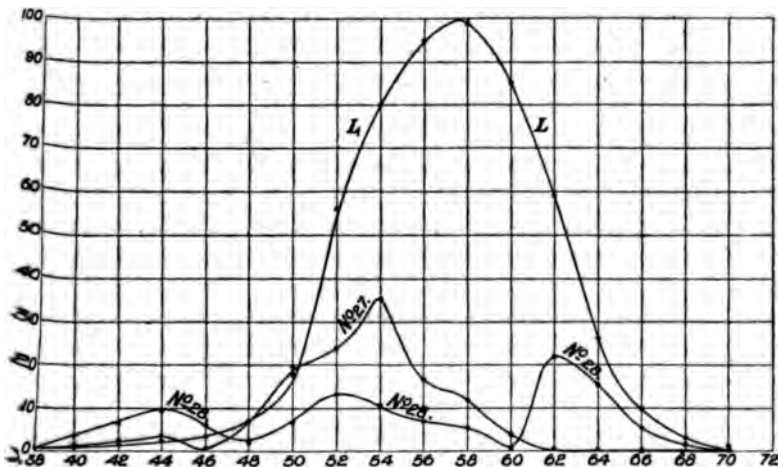
The total excess of white is about the same, but the distribution not so good, as the ends of the spectrum, when the luminosity is small, have large excesses.

The absorptions of the above inks are—

Yellow.		Fink.		Blue.	
Violet.....	135·6	Violet.....	12·8	Violet.....	0·2
Green.....	10·6	Green.. ...	105·2	Green.. ...	33·0
Red	5·1	Red	55·9	Red	87·3



No. 28.—Filter curves for inks with abrupt absorptions at λ 50·5 and λ 57·5.



No. 29.—Curves showing the excess of white which the filters will give in the finished print of the spectrum with the theoretical inks. LL, curve showing luminosity of the spectrum.

The results of the calculations are exhibited in the accompanying diagram, curve No. 28, and the distribution of the excess white which they involve is shown in the diagram, No. 29, together with Abney's

luminosity curve of the normal spectrum. It is obvious from the fact that inks whose absorptions terminate at λ 49 and λ 59 will be better than those terminating at the points of intersection of the ray composition curves.

PART III.—EXPERIMENTAL.

15. *The Apparatus.*

Throughout the following experiments we used a copy of Abney's colour-patch apparatus. The only difference in the main instrument was the use of a right-angle prism in front of the collimator slit to reflect the light from the lantern down the tube. This enabled us to set the apparatus up much more nearly in a direct line, and therefore to arrange it all on an ordinary long table.

The collimator was about 13 inches long, with a $2\frac{1}{2}$ -inch achromatic lens. The slit was one of Elliott Brothers'.

The prisms were equilateral prisms cut from a very fine specimen of white glass especially selected for this purpose. The face of each was about $2\frac{1}{2}$ by $2\frac{1}{2}$ inches.

The projecting lens was an achromatic one, $2\frac{1}{2}$ inches diameter and about 30 inches focus. It produced a visible spectrum 8 cms. long which was very bright and sharp.

The slit was mounted in a brass frame arranged to slide horizontally across the spectrum in its focal plane. It was about $2\frac{1}{2}$ inches high and usually about .5 mm. open. A small 1-inch right-angle prism, carried by the same frame, was arranged to reflect the light from the top half of the slit to one side on to a second and larger prism, also carried on this frame, which reflected it forward.

Combining Lenses.—These were two 6-inch lenses of about 20 inch focus, one of which received the light from the lower half of the spectrum and focussed it on the patch. This is the direct beam. The other lens focussed the reflected beam on the same patch. This patch showed the image of the aperture of the double-image prism mentioned below. The distances of the lenses were adjusted until the horizontal edges of the patch were sharp, and were then rotated slightly round a vertical axis until the patch remained stationary as the slit was moved across the spectrum.

The patches were formed by cutting a small square window in a piece of black velvet pasted on a card. To avoid stray light the card was placed at the far end of a box, 2 feet long, blacked all over. The mouth of the box was covered with brown paper, and holes were cut in this just large enough to admit the two beams. A part of one side was removed to enable the patch to be observed. A blackened pillar was placed, as in the Rumford photometer, to obtain a sharp dividing line, one-half of the patch being lighted

the direct beam and the other half by the reflected beam. The two patches were adjusted (before the pillar was inserted) to exactly coincide, so that any want of uniformity in the illumination might affect them equally; this we found most important. The patches were slightly larger than the window, so that it was lighted up to its edges. The pigment to be compared was always placed in the reflected beam, the white beam falling on a piece of the same paper that the pigment had been printed upon.

The sector was one made by Hilger upon the lines laid down by Abney. It closes entirely, and opens to one-half of the full beam. The aperture is divided into 100 parts, so that percentage aperture can be read directly. It has to run at a high speed to obtain good results. I found that there was a lot of backlash in the sleeve, amounting to about two divisions. To avoid the error due to this, it must be stopped always in the same way. I found by exactly closing the sector while it is running—which can be done very accurately by looking through it to the light and adjusting it until the light is entirely extinguished—and then stopping it *by the sleeve*, the reading is nearly always zero. If in addition the handle has last been moved in the direction to *close* the aperture, the reading was always found to be correct. These experiments took a long time. The sector was placed in the direct beam just in front of the box spoken of above.

As the direct beam is cut down to one-half by the sector, even when the latter is fully open, it is necessary to reduce the reflected beam also to enable a balance to be obtained. This was done by attaching a Nicol's prism to the frame carrying the slit and the two right-angled prisms in the beam between the two latter. As the light has already been polarised by the double-image prism, it is possible by rotating this Nicol to reduce its luminosity to any desired extent.

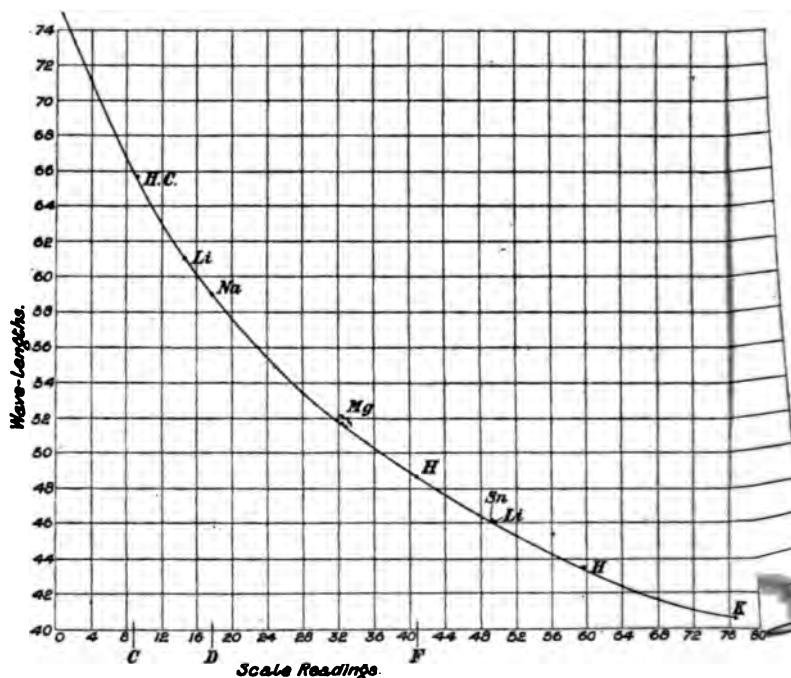
The double-image prism was about 1 inch square aperture. It was at first mounted against the collimator lens, but finally was placed against the projecting lens. Its adjustment is of very great importance, and it was found impossible to set it with sufficient accuracy by hand. It was therefore mounted in a brass frame which could be given a very slow rotation by means of a screw, and after the prism had been adjusted as nearly as possible by hand, it was finally corrected by this screw. Its rotation causes the two spectra, into which it divides the original one, to move relatively across one another, and therefore varies the colour which passes through the slit. The prism is rotated until, with the slit in the yellow, the two beams are exactly the same colour. As the slightest alteration here is very easily perceived, the adjustment can be effected very accurately, and will then be correct throughout the spectrum.

A glass Zeiss millimetre scale was temporarily attached to the top of the camera, and a pointer on the frame carrying the slit moved

along the scale and indicated the position. The D line was at 50 on this scale. The temporary scale was afterwards replaced by a permanent one on which the D line was at 17.7, and to avoid confusion, the early scale readings have been reduced to this scale by subtracting 32.3 from each.

16. *Scaling.*

For this, the slits were removed and the spectrum was received on a strip of celluloid placed between a glass plate and a glass Zeiss millimetre scale. Various substances were then placed on the arc, such as Li, K, Na, Mg, and the bright lines read. A H-tube was also placed at the collimator slit. The results are plotted in Curve No. 1 :—



No. 1.—Curve connecting the scale readings with the corresponding wave-lengths.

Line.	Scale.		Wave-length.
	Old.	New.	
.....	50·0	17·7	5889
sd.....	39·8	7·0	6708
range	46·8	14·5	6102
lue	81·8	49·5	4603
d.....	30·5	1·8	7680
olet.....	109·0	76·7	4045
.....	41·2	8·9	6563
.....	73·0	40·7	4860
.....	92·8	59·5	4340
green	64·1	31·8	5183
.....	64·4	32·1	5172
.....	64·5	32·2	5167
ue	81·6	49·3	4607
.....	56·5	24·2	5536
.....	57·5	25·2	5492
.....	37·1	4·3	6933
.....	56·3	24·0	5543
.....	98·3	66·3	4226

D line on the new scale was at 17·7, and as it was also a milli-scale, the new scale readings will all be 32·3 less than the old ones.

For this the following readings were taken :—

Stem.	D.	Li, red.	Li, blue.	Mg.
scale.....	50·0	39·3	81·8	64·5
scale	17·7	7·8*	49·5	32·3
rence.....	32·3	31·4	32·3	32·2

17. Determination of the Curves of Practical Inks.

May 1, 1900.—We set up the apparatus with the two right-angle prisms and obtained the patches. We found some difficulty due to light, which we nearly eliminated, so far as the outside of the apparatus was concerned, by inclosing it with screens. A cloth was hung over the arc and over the collimator and prisms, and a red canvas screen was hung up to divide the end of the room. When the observations were made from the end containing the light source, the matching pigments in a part of the spectrum in which they appeared nearly black, we were still able to perceive a small difference of intensity in the two patches, which was due to internal reflections on the

* Probably an error in reading.

collimator and camera bodies and at the surface of the double-image prism. We reduced this as far as possible by the liberal use of stops, but could not entirely avoid it. This is not wonderful when we consider the small luminosity of the ends of the spectrum, and remember that we were often only using 2 or 3 per cent. of this feeble light. It is obvious that a very small amount of reflected light would be easily noticed. On some occasions the patches were observed through coloured gelatines to help to cut this off.

For these adjustments I am greatly indebted to my friend, Mr. Alex. A. Tallent, who worked most enthusiastically in getting the apparatus together, and who has assisted me throughout in the experiments.

The relative intensity of the direct and reflected beams was not constant through the spectrum, and it was necessary to measure it. For as the light strikes the second refracting prism at an angle very near the polarising angle, it is thus partly polarised when it reaches the double-image prism, and therefore the two beams into which this prism splits it will not be of equal intensity, and this inequality is more marked for some colours than for others.

March 27.—I measured a yellow and red ink by Messrs. Mander, of which I had made rubbings. Probably due to small deposits of moisture which form on the prisms and lens—owing apparently to the glass being more than usually hygroscopic—the inequality above mentioned varies from day to day. The moisture probably reduces the polarisation by reflection at the surface of the prism, and thus alters the relative luminosity of the beams. It is not advisable to clean the surfaces very often for fear of spoiling the adjustments. It is therefore necessary to frequently re-determine the relative brightness of the patches.*

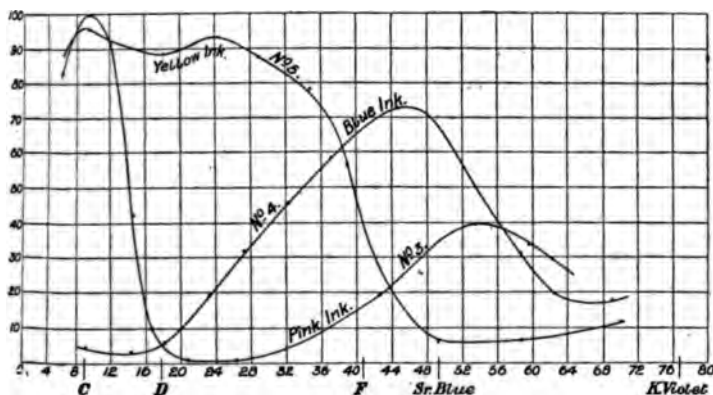
We found a great improvement in the accuracy of our readings as we became more experienced, and also as the stray light was more perfectly eliminated. The chief difficulty remaining was caused by the unequal density of the inks over the patch, which is far more obvious when measuring them in a monochromatic light than it is when casually observed in ordinary light.

18. *Description of the Curves.*

With this apparatus I measured the proportions of the light of each colour throughout the spectrum that was reflected by several of the inks sold commercially for three-colour printing.

I measured such a set of inks sent by Messrs. Fleming and Co., and described as "Theoretical Inks" for three-colour work. They are not permanent, but that would not be important for book illustration,

* The apparatus was entirely re-adjusted after No. 2 was measured.



Curves from Fleming's "Theoretical" Inks.

though it would be fatal for advertisements or pictures to be exhibited.

Jan. 2.—The Pink Ink No. 1303 (Curve No. 3) seems very satisfactory, as its absorption begins very rapidly in the orange, whilst the curve rises again fairly high in the blue.

The Blue Ink No. 1265 (Curve No. 4) is far too opaque in the yellow and green. The absorption commences very gradually, and there is also an absorption at the violet end. The ink thus leaves much to be desired. It should rise at about 18 (scale) to 100 per cent., and stay at that to the end, and this it is very far from doing.

The Yellow Ink No. 1271 (Curve No. 5) is very transparent well into the blue, and will probably do well.

Messrs. Fleming also sent me a set of rubbings of the inks they ordinarily supply for photochrome work. These inks are permanent or nearly so:—

Photochrome Red No. 1197 (Curve No. 6) has an abrupt absorption rather further in the yellow than the "Theoretical Red," and is so far better, but is far less transparent in the blue, rising at the blue lithium line only to about 5 per cent. as against the 35 per cent. of the "Theoretical Red."

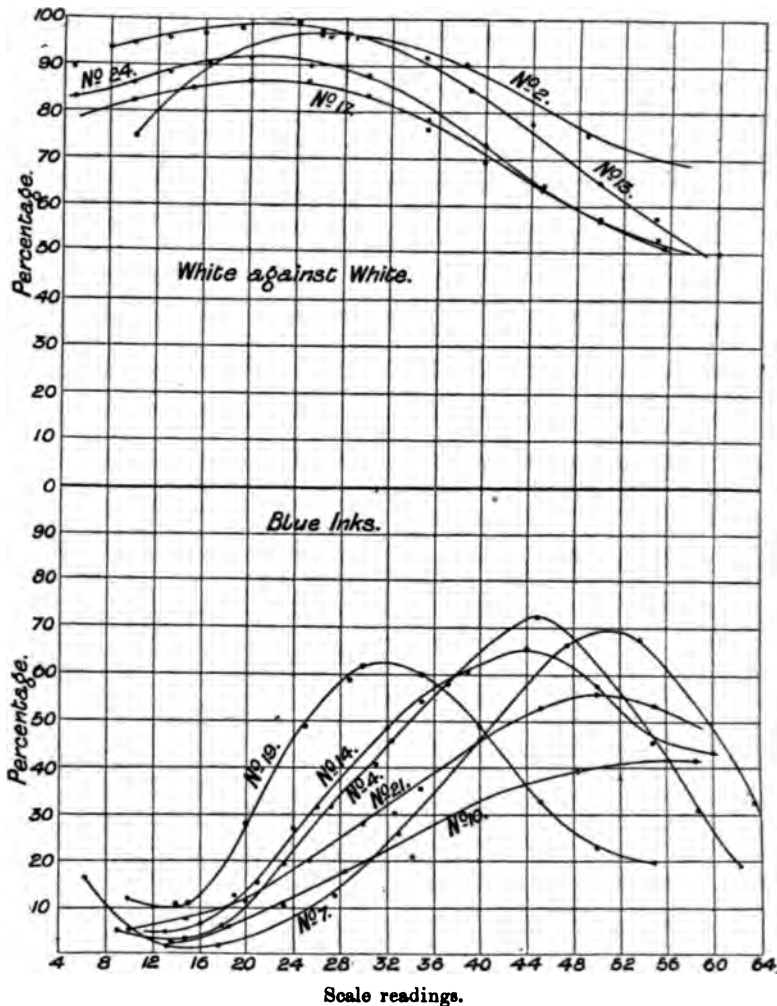
Photochrome Blue No. 1199 (Curve No. 7) rises fairly rapidly, but much too late, namely, in the blue-green instead of the yellow. It will not be possible to make bright greens with this ink.

Photochrome Yellow No. 1198 (Curve No. 8).—The absorption is more abrupt than that of the previous yellow, but it is earlier, that is to say, it is not sufficiently transparent to the blue. It ought to occur at about 46 or 50 instead of 32 (scale).

I measured a set of inks which were printed for me by Mr. Gamble, of the Bolt Court School of Photo-lithography. The pink ink (Curve

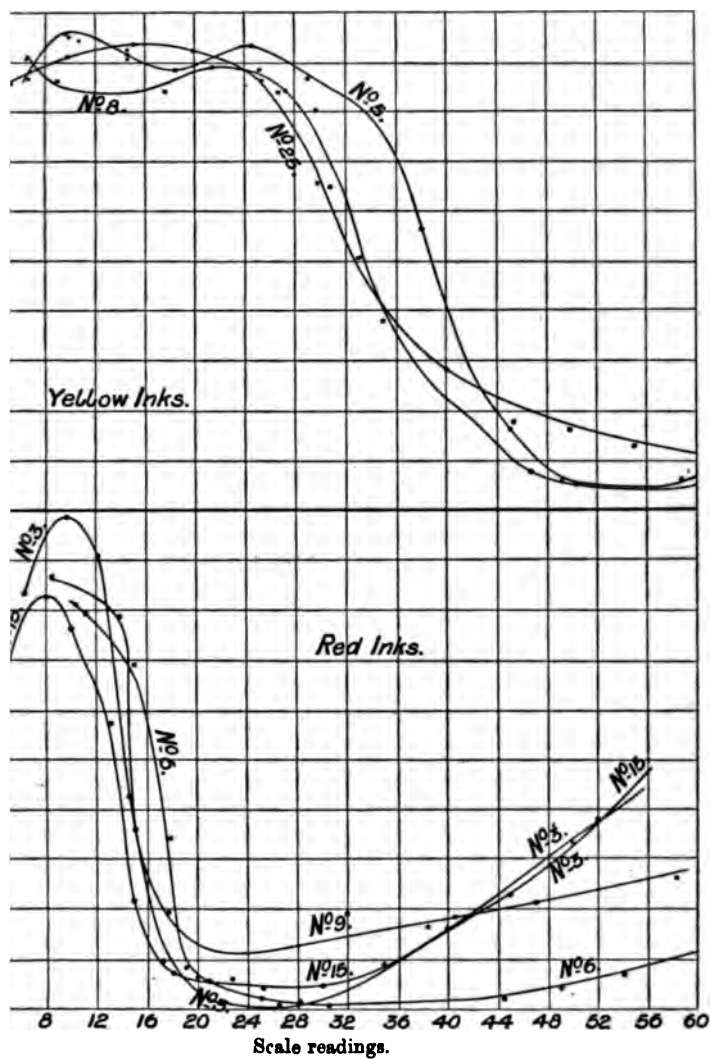
No. 9) had no very great absorption anywhere. The blue ink (Curve No. 10) was not transparent enough anywhere, especially in the green.

In another set of inks printed by Mr. Gamble the blue (Curve No. 14) had very much the character of the first one above, as had also the pink and yellow (Nos. 15 and 18).



The White against White Curves show the effect of the film of moisture on the prism, on different dates.

Mr. Gamble formed a *Red* by printing both the pink and yellow inks on the same paper, and it is interesting to compare the absorptions



red so obtained with the absorptions calculated from those of
 ink and yellow inks. As it will at the same time show how the
 were obtained in the other cases, the following table is
 1:—

Scale.	Average aperture for yellow ink.	White.	Per cent.	Per cent. for pink ink.	Product of percentages.	Per cent. for red ink from experiment.
10	78.5	82.0	95.7	76.7	73.3	75.7
15	79.0	85.0	93.0	21.9	20.4	20.3
20	79.5	86.5	92.5	6.37	5.9	4.5
25	77.0	86.2	88.5	4.4	3.9	5.7
30	67.7	83.5	80.0	4.7	3.76	5.8
35	34.0	76.5	44.5	9.0	4.0	6.3
40	15.0	69.5	21.6	16.2	3.5	7.4
45	7.7	64.4	11.94	23.0	2.75	7.1
50	6.0	57.3	10.46	33.4	3.5	6.2
55	4.5	50.5	8.9	46.6	4.16	6.1

In the above table the column headed "Average aperture for yellow ink" gives, for each spectrum colour, the aperture of the sector which made the light reflected from the white patch equally bright with that reflected from the patch printed with the yellow ink. The next column, headed "White," gives the aperture of the sector which made the two patches equal when both were white. This column would have been constant but for the polarisation by reflection at the surface of the refracting prism already referred to. From these two columns the proportion of the light reflected by the ink as compared with that reflected by white paper can be calculated. The result appears in the column headed "Per cent."

In the same way the percentage light reflected by the pink and the red patches were found, and are entered in the fifth and seventh columns. In the sixth column I have calculated the light which should be reflected from a paper printed with both the yellow and the pink inks. The agreement is very fair. The actual red reflects more in all the darker parts than calculated. This may be due to the yellow not being quite so heavily printed, but it is more probably because the pink ink, which was uppermost, was not quite transparent, and some light was reflected directly from it instead of from the white paper beneath. Such light would not suffer by the absorption of the yellow ink.

The "red" resulting from these printings has a very orange hue, as was to be expected.

A yellow ink by Mander, No. 226 (Curve No. 25), was less heavily printed than those previously examined, and therefore seems better in the green and blue green, but the absorption in the violet suffers in consequence.

A pink ink by Mander, No. 0227 (Curve No. 26), comes up fairly in the violet, but is not opaque enough in the green.

As the worst ink was always the Blue, I attempted to find a colour which should be more transparent in the green. I obtained a bright green rubbing from Fleming, but I found it was green not because it was more transparent in the green and yellow-green, but because it was more opaque in the blue and violet. There seems to be no ink in the market at present that is sufficiently transparent in the yellow-green.

19. Attempt to find the Effect of a Difference in the Depth of the Colour.

The intensity of the reflected beam is given (for any one colour) by $I' = Ia^e$, where e is the depth and I is the original intensity of the beam. For another depth of tint f

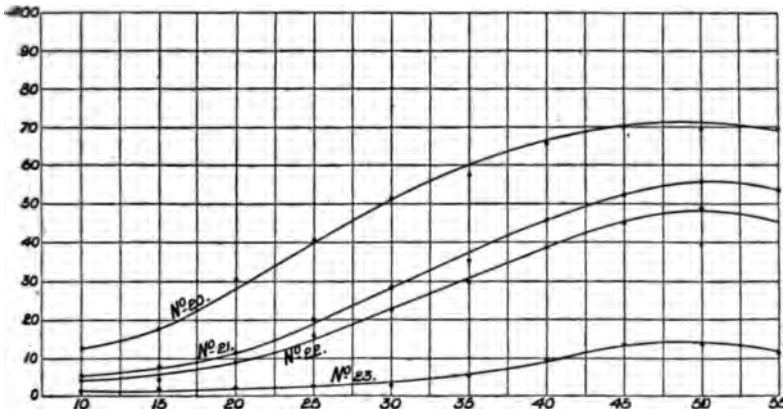
$$I'' = Ia^f,$$

$$\log \frac{I'}{I} = e \log a, \quad \log \frac{I''}{I} = f \log a,$$

$$\therefore \frac{e}{f} = \log \frac{I'}{I} \div \log \frac{I''}{I}.$$

Thus the ratio of the log ought to be a constant through the spectrum. Neglecting the readings in which the aperture was too small to be taken accurately (owing to the backlash of the sector), and in which also the scattered light is of importance, the following results show a very close agreement with theory.

The inks were only rubbed on with the finger, so that the patches were not exactly uniform, especially the lightest one (1). In this case



Curves showing the absorptions of the same ink printed to different depths.

the readings were checked by drawing a smooth curve between the points found, and the numbers taken from the curve, where they were

obviously wrong. Otherwise the actual means were used so that the error might be the more readily estimated. Had I been able to obtain printed uniform patches I have no doubt the results would have been better. But even these can leave no doubt of the approximate truth of the law.

A Blue Ink from Winstone.

Curve No. 20.—I. A very faint rubbing.

Scale.	Aperture.	Average.	Corrected from curve.	White.	Per cent.	Log of ratio.
10	10·0 — 10·5	10·2	—	81·2	12·4	—0·906
15	16·0 — 14·5	15·2	—	85·4	17·7	—0·752
20	27·0 — 27·0	27·0	—	87·0	31·0	—0·508
25	35·5 — 35·0	35·2	—	86·6	40·6	—0·392
30	43·0 — 43·0	43·0	—	83·5	51·6	—0·288
35	43·0 46·0 45·0	44·6	—	77·2	57·8	—0·238
40	45·0 — 46·0	45·5	—	69·3	65·7	—0·182
45	50·0 53·0 48·50	50·0	—	64·4	70·0	—0·155
50	38·0 42·0 39·0	39·6	—	57·3	69·1	—0·160
55	38·5 — 38·0	38·2	—	54·7	69·7	—0·157

Curve No. 21.—II. A little darker than I.

Scale.	Aperture.	Average.	Corrected from curve.	White.	Per cent.	Log of ratio.
10	4·5 — 4·0	4·2	—	81·2	5·2	—1·384
15	6·0 — 6·5	6·7	—	85·4	7·8	—1·107
20	10·5 — 9·5	10·0	—	87·0	11·5	—0·939
25	18·0 — 16·5	17·2	—	86·6	20·0	—0·699
30	23·0 — 24·0	23·5	—	83·5	28·1	—0·551
35	27·5 26·5 28·0	27·3	—	77·2	35·5	—0·450
40	32·0 — 32·0	32·0	—	69·3	46·0	—0·337
45	33·0 35·0 33·0	33·6	—	64·4	52·2	—0·282
50	27·0 28·0 32·0	29·0	32	57·3	55·8	—0·254
55	30·0 29·5 —	29·8	—	54·7	53·4	—0·272

Curve No. 22.—III. Still darker rubbing.

Scale.	Aperture.	Average.	Corrected.	White.	Per cent.	Log of ratio.
10	3·5 — 3·2	3·3	—	81·2	4·07	-1·391
15	4·0 — 4·0	4·0	—	85·4	4·68	-1·329
20	7·5 — 8·0	7·8	—	87·0	9·07	-1·042
25	18·5 — 18·5	18·5	—	86·6	15·6	-0·806
30	19·0 18·2 20·0	19·0	—	83·5	22·8	-0·642
35	23·0 — 23·2	23·1	—	77·2	29·9	-0·524
40	26·0 28·0 27·0	27·0	—	69·3	38·9	-0·410
	28·5 — 26·5					
45	8·1 — 28·5	29·0	—	64·4	45·0	-0·346
50	28·0 — 28·0	21·0	—	57·3	48·8	-0·312
55	21·0 — 22·0	21·5	—	54·7	39·3	-0·406

Curve No. 23.—IV. Very heavy rubbing of the same ink.

Scale.	Aperture.	Average.	Average corrected from curve.	White.	Per cent.	Log of ratio.
10	1·5 — 1·5	1·5	1·5	81·2	1·85	-1·732
15	2·0 — 2·0	2·0	1·9	85·4	2·12	-1·674
20	2·0 — 2·0	2·0	2·1	87·0	2·42	-1·616
25	1·5 — 1·7	1·6	2·2	86·6	2·55	-1·594
30	2·5 — 2·3	2·4	—	83·5	2·88	-1·541
35	3·8 — 4·0	3·9	—	77·2	5·06	-1·296
40	6·1 6·6 6·2	6·5	—	69·3	9·37	-1·028
45	8·5 — 8·8	8·6	—	64·4	13·35	-0·874
50	8·2 — 7·5	7·9	—	57·3	13·77	-0·861
55	6·5 — 6·5	6·5	—	54·7	11·85	-0·926

Scale.	III I Ratio $\frac{e}{f}$.	IV III Ratio.	III II Ratio.
10	—	—	—
15	(1·77)	—	(1·00)
20	2·05	—	1·11
25	2·05	(1·98)§	1·15
30	2·24	2·41	1·16
35	2·20	2·42	1·16
40	2·27	2·51	1·21
45	2·26	2·54	1·23
50	1·95*	2·76*	1·23§
55	(2·61)	2·27	(1·48)
Mean	2·16	2·45	1·18

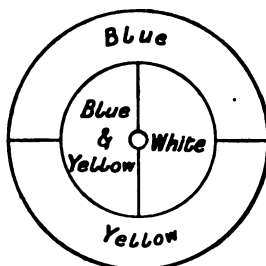
§ These two are the only ones which have been corrected from the curve.

60 *Application of Maxwell's Curves to Three-colour Work, &c.*

These ratios (omitting the ones in brackets where the lack of light made the readings too uncertain) do not differ from the mean by more than one in eight, and omitting the ones marked *, the difference is not more than one in fourteen.

20. *Experiment to see whether the Colour produced by Blue and Yellow side by side matched that when they were printed on one another.*

Mr. Gamble prepared for me a card printed with blue and yellow side by side. Then also a third printed with both the blue and yellow; this, of course, made green. I cut the blue and yellow into discs about 8 inches in diameter, and the green into one about



4 inches in diameter. Also I cut a white—the white on which they had all been printed—into a disc 4 inches in diameter. Then I placed them on a motor, as in Maxwell's discs, with equal areas of blue and yellow, and of green and white. These ought when rotated to match if the inks are to be used for process work. I found there was a very great difference in both hue and luminosity. To make a match it was necessary to reduce the yellow to only 125° and increase the blue to 235°, and by making the white 125° and the green 235° the match was correct in luminosity as well as hue.

Thus, as would be expected since the inks had not abrupt absorptions, it made a very great difference if the colours were superposed or were side by side.

“On the Seasonal Variation of Atmospheric Temperature in the British Isles and its Relation to Wind-direction, with a Note on the Effect of Sea Temperature on the Seasonal Variation of Air Temperature.” By W. N. SHAW, M.A., F.R.S., Secretary to the Meteorological Council, and R. WALEY COHEN, B.A. Received June 11,—Read June 20, 1901.

The following paper is mainly concerned with the analysis of the seasonal variation of temperature of the British Islands into a series of simple harmonic curves. The variation of temperature is so irregular that the use of this method of analysis for the investigation of the subject may seem to be arbitrary and inappropriate, and a few words of introduction are accordingly necessary to indicate the circumstances under which this mode of dealing with the subject showed itself to be specially adapted for the purpose.

Observations have been made for the Meteorological Office since 1871 with self-recording instruments at the four observatories, Kew, Aberdeen, Falmouth, and Valencia, and the daily means of pressure and temperature from the twenty-four hourly readings of the curves of the barometer and dry-bulb thermometer have been worked out for twenty-five years, but have not yet been published. The means for temperature are represented in Diagram 1 (p. 62), by curves whose co-ordinates are respectively proportional to the number of days since the beginning of the year, and the corresponding twenty-five-year mean of the day's temperature at the several observatories.

The following points may be noticed upon an inspection of the curves :—

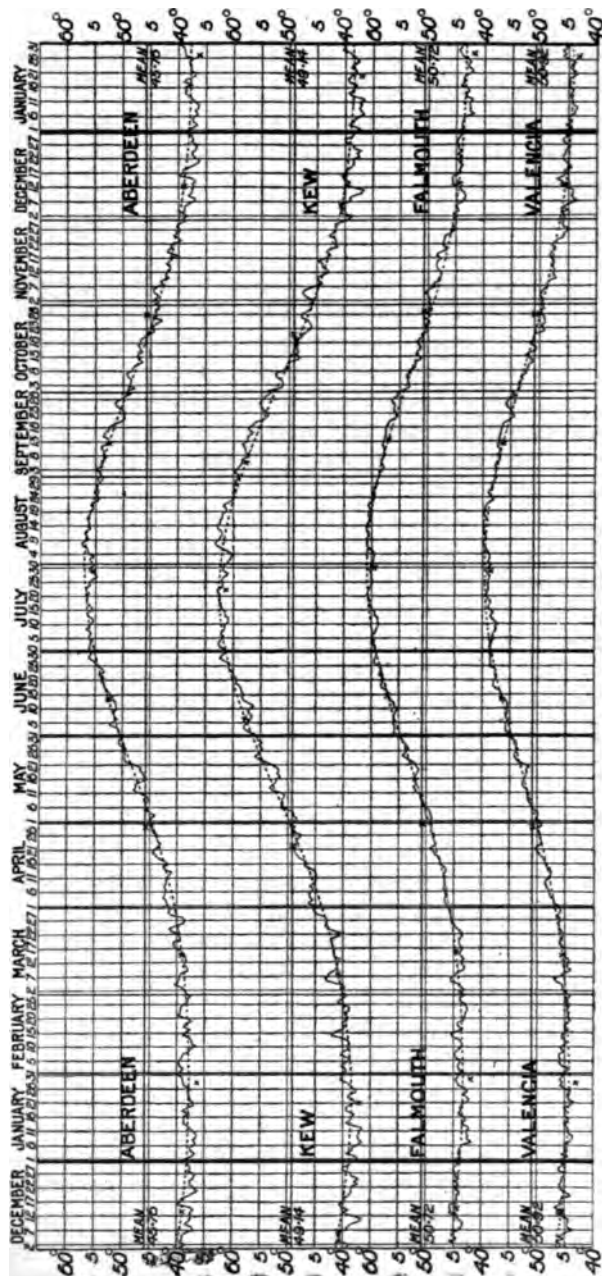
(1.) The summer portions of the curves have a larger amplitude and a shorter duration than the winter portions.

(2.) The curves do not exhibit a smooth run, but show a number of irregularities which are, in some instances, particularly in May and December, of considerable magnitude.

Under these circumstances it is difficult to assign any specific number as the normal mean temperature for the particular day to which any actual observed temperature can be referred. To put the same point in other words, it is difficult to say whether a depression between two elevations is to be regarded as a period of abnormally low temperature between two normal periods, or as a period of normal temperature between two abnormally high periods.

It will be noticed that on the diagram smoothed curves have been drawn, which represent with remarkable fidelity the general sweep of the curve, cleared of the irregularities of small period. The smoothed

MEAN TEMPERATURE OF EACH DAY OF THE YEAR. 25 YEARS 1871-1895.



curves are not drawn by eye merely to smooth out the irregularities, but they have been arrived at by a process of synthesis of simple harmonic curves of the first and second orders.

The irregular lines were first compared with a simple harmonic curve of an annual period, drawn so as to give the same area as the irregular line above and below the line of mean annual temperature, and a general lag of about a month of all the seasons behind the planetary* seasons. The comparison at once disclosed that apart from the irregularities of short period, there was a lag of spring and an acceleration of autumn, and a corresponding exaggeration of the summer maximum and moderation of the winter minimum. This result, which is essentially characteristic of the combination of a second-order sine curve with one of the first order having a maximum at the same epoch, suggested the idea that a normal curve of reference could be formed by combining two such curves.

The first-order sine curve for Kew was accordingly placed so as to make the lag of spring and the acceleration of autumn, as roughly estimated, equal; under these circumstances, the maximum fell on July 23, which coincides satisfactorily with the maximum of the original curve. The second-order sine curve was compounded with this—the period of the second-order curve being six months—and its amplitude† was so arranged as to produce the observed lag of spring and acceleration of autumn; as these had been taken to be equal to each other, the maximum of the second-order curve, and therefore of the compounded curve, corresponded with that of the first-order curve and of the original smoothed curve. A similar process was followed for the other three stations. It is apparent that these new compounded curves give very satisfactory smoothed curves for the whole year for each of the stations. Thus it would appear that the regular periodic variations of mean atmospheric temperature at Kew may be practically represented by the summation of two effects, one of which corresponds with a sine curve having an amplitude of $12^{\circ}04$ F., and a period of one year, and the other with a sine curve having half the period and an amplitude of $1^{\circ}4$ F. Similar statements with suitable numerical magnitudes are true of the other stations. These results, which had been reached synthetically, were confirmed analytically within narrow limits,‡ and gave rise to the conclusion that there is in the twenty-five-

* The word "planetary" is used in this paper to characterise a variation corresponding to the position of the earth in its orbit.

† The relation between lag and amplitude is as follows: The double amplitude of the second-order curve = annual range $\times$ sine of angular lag.

‡ The dotted lines in Diagram 1 show the combination of the synthetic curves of the first and second order; the crosses mark points on the curve of the first order. A comparison with Table I shows that a sensible error was introduced by assuming the maxima of the first- and second-order components to be synchronous in the case of Kew and of Aberdeen.

year temperature curve of each of the British stations, a perfect regular and recognisable periodic effect, which has positive maxima at the end of January and the end of July, and negative maxima about the end of April and the end of October. It will be seen later on (pp. 65, 66, and 77) that this curious and well-marked effect with a period of six months, having maxima about the end of January and July, is characteristic only of the British Isles, among the places where temperature statistics have been investigated from that point of view. It is equally conspicuous in the variation of sea temperatures, and particularly marked in the variation of barometric difference between London and Aberdeen (see Table III, p. 66).

This paper is devoted to a further discussion of this effect, and an attempt to trace its cause. As the process of harmonic analysis gave results for the twenty-five years which corresponded with the synthetic examination of the curves, it seemed better to deal with the observations in the better recognised and more rigorous way.

Harmonic Analysis of Twenty-five-year Mean Values of Daily Temperature at Kew.

The mean temperature of any day may be represented by the following equation

$$\theta = a_0 + P_1 \cos (x - \mu_1) + P_2 \cos 2 (x - \mu_2) + \dots,$$

where a_0 is the mean value of the curve for the year, and $P_1, P_2, \dots$ represent the amplitudes of the first, second, and higher order curves, while $\mu_1, \mu_2, \dots$ represent the period of the year, expressed in degrees since the beginning of the year, at which the first maximum of each curve occurs. From this equation, by means of a formula which has been worked out by Sir R. Strachey,* the values of these harmonic coefficients $P_1, P_2, \&c.$, and $\mu_1, \mu_2, \&c.$, have been determined in the Meteorological Office from the twenty-five-year means of the five-year mean temperatures at Kew, Aberdeen, Valencia, and Falmouth. The results of this analysis are shown in Table I. It will be seen that in each case there is a second-order curve whose amplitude is about one-eighth of that of the first-order curve, and that the amplitudes of curves of a higher order are generally so small as to be negligible, and moreover are very much more variable than the values of the coefficients of the second-order curve.

Thus it appears that the twenty-five-year mean curve of daily temperature indicates the existence of two effects, represented respectively by a first-order and a second-order curve. The first-order curve presents a primary solar effect, with which we are not here concerned. The purpose of this investigation has been to ascertain the nature and physical cause of the second-order effect.

* 'Roy. Soc. Proc.,' vol. 42, pp. 61-79.

HARMONIC ANALYSIS OF CURVES.

Table I.—Atmospheric Temperature.

Station.	Period observed.	Mean temp.	Length of summer part.	1st-order curve.		2nd-order curve.			3rd-order curve.		4th-order curve.	
				Ampli- tude.	Date of max.	Ampli- tude.	Date of 1st max.	Date of 2nd max.	Ampli- tude.	Date of max.	Ampli- tude.	Date of max.
Valencia....	25 years, 1871 to 1895	° F. 50·9	days. 169	° F. 7·61	July 29	° F. 1·07	Jan. 30	July 31	° F. 0·35	Jan. 18	° F. 0·07	Mar. 3
Aberdeen ..		45·8	165	9·44	July 28	1·45	Feb. 4	Aug. 5	0·26	Feb. 14	0·12	Jan. 28
Falmouth ..		50·7	167	8·83	Aug. 1	1·18	Jan. 28	July 30	0·15	Feb. 2	0·10	Feb. 23
Kew		49·1	170	12·08	July 23	1·38	Feb. 3	Aug. 4	0·04	Feb. 16	0·12	Feb. 19
"		49·17	163	11·62	" 25	1·95	" 4	" 5	0·45	Feb. 28	0·32	Mar. 7
"		48·94	163	11·94	" 24	2·06	" 9	" 10	0·67	Mar. 3	0·30	" 11
"		49·17	164	11·10	" 26	1·84	Jan. 29	July 30	0·22	Jan. 15	0·32	" 2
"		50·52	146	10·99	" 26	3·57	Feb. 6	Aug. 7	1·08	" 16	0·61	Jan. 10
"		49·3	159	10·95	" 26	2·83	Jan. 16	July 17	1·38	Apr. 15	1·83	Apr. 3
"		49·1	174	14·80	" 24	1·51	Apr. 22	Oct. 21	2·45	Mar. 23	2·30	Mar. 17
"		51·1	176	11·23	Aug. 5	1·38	Jan. 27	July 28	1·50	Jan. 4	0·78	Jan. 31
Oxford	1 year, 1899	50·4	154	12·88	July 12	3·18	Jan. 14	July 15				
Vienna	100 years	49·1	188	19·82	July 18	0·99	Mar. 24	Sept. 24	0·32	Jan. 2	0·41	Feb. 12
Agra	7 years, '68-'74	78·8	198	15·51	June 29	5·22	Apr. 27	Oct. 25	1·14	Feb. 4	0·39	Mar. 11

Table II.—Sea Temperature.

Station.	Period observed.	Mean temp.	Length of summer part.	1st-order curve.		2nd-order curve.		3rd-order curve.		4th-order curve.	
				Ampli- tude.	Date of max.	Ampli- tude.	Date of 1st max.	Ampli- tude.	Date of max.	Ampli- tude.	Date of max.
Shetlands	Air	—	—	° F.		° F.		° F.		° F.	
	Sea	45	176	7.55	Aug. 7	1.30	Jan. 29	0.25	Jan. 13	0.23	Feb. 1
Scilly	Air	—	—	7.00	" 12	0.89	Feb. 11	0.43	Jan. 11	0.04	Mar. 4
	Sea	54	176	8.00	Aug. 7	1.16	Feb. 3	0.01	Apr. 19	0.09	Feb. 24
Yarmouth	Air	—	—	6.01	" 10	0.86	" 10	0.19	Apr. 31	0.09	" 1
	Sea	49	175	11.96	July 30	1.34	Feb. 13	0.16	Mar. 5	0.10	Feb. 14
				10.55	Aug. 13	1.28	" 21	0.03	Mar. 27	0.31	Jan. 6

Table III.—Barometric Gradient (in hundredths of an inch). (Reduced to M.S.L., and 32° F.)

London — Valencia London	30 years, 1871 to	3.07	Jan. 1	1.33	Jan. 16	July 17	0.9	Jan. 15	0.5	Feb. 4
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The Effect of the Types of Weather (Cyclonic or Anticyclonic) on Temperature during the Five Years 1876-1880.

The period from 1876 to 1880 was first examined. With the object of classifying the days of this period, the atmospheric conditions prevailing on each day were recorded on a separate card, to be subsequently grouped in any manner that might be thought desirable. The conditions recorded were—

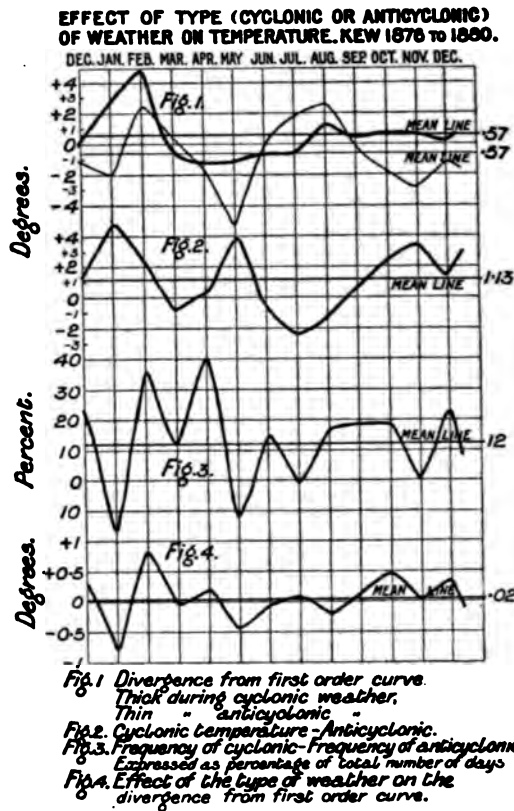
1. The mean temperature.
2. The direction of the wind.
3. The weather.
4. The direction of the gradient (high to low), and
5. The type of the weather prevailing (cyclonic or anticyclonic).

For this purpose weather was set down as cyclonic if the isobar on the 8 A.M. chart was concave to a region of low pressure, and as anticyclonic if the isobar was concave to a region of high pressure. The classification was made according to the type of weather. It was thought possible that this second-order temperature effect might be a result of periodic variations in the relative frequency and effect of cyclonic and anticyclonic weather at different periods of the year. With the object of ascertaining whether this was the case,* the mean temperatures and frequencies of occurrence of cyclonic and anticyclonic weather, during each month of the year, were tabulated for the five years under observation. For the purpose of examining the results, curves of temperature were drawn whose ordinates are proportional, not to the absolute mean temperature for the month, but to the difference between the mean monthly temperature and the temperature which would be represented by the mean of the ordinates of the first-order curve during the month; so that the resulting curves show at part of the temperature variations which is represented in the harmonic analysis by a summation of all the curves of higher order than the first. Diagram 2, fig. 1 (p. 68), shows these curves of temperature for the cyclonic and anticyclonic days respectively. It will be seen that only during the three summer months, and to a slight extent during March, is the cyclonic weather cooler than the anticyclonic. But both the curves show the main characteristics of the second-order curve, viz., maxima in February and August, and minima in May and November. Moreover, the curve of difference of temperature between cyclonic and anticyclonic weather (fig. 2) shows three maxima in the year. Fig. 3 shows the differences in frequency of the two types of weather during each month, expressed as percentages of the whole number of days in the month; here again the curve is irregular, having

* Mr. W. H. Dines, 'Quart Journ. Roy. Met. Soc.,' vol. 23, p. 237, has already given some reasons against regarding cold as definitely associated with anticyclonic weather in winter.

several maxima which occur at irregular intervals, showing that the relative frequency of the two types of weather plays no part in causing the second-order effect. Finally, by multiplying the percentage difference of frequency of cyclonic and anticyclonic weather for each month by the difference in temperature (excess of the cyclonic, in each

DIAGRAM 2.



being taken as positive, and *vice versa*), a resulting curve (fig. obtained which gives the total effect of the type of weather or atmospheric temperature. A glance at this curve will show that effect is in no respect similar to the second-order effect. It is evident that the cyclonic or anticyclonic type of the weather play part in causing the second-order effect, which is nevertheless the prominent periodic temperature variation which has a meteorological origin.

The Effect of Wind-direction on Atmospheric Temperature during the Five Years 1876-1880.

A second classification was then made in which, while the types of weather were still distinguished, curves of the difference between first order curve values and mean daily temperature observed during the prevalence of barometric gradients for each of eight points of the compass, were separately drawn. Each of these curves showed certain characteristics which corresponded with some of those of the second-order curve; either the February maximum or the May minimum or some other characteristics were prominent, but no single one of the curves for the separate gradients showed a complete curve with all the characteristics of the second-order curve. It was, however, very remarkable that the curves for the same winds during different types of weather were closely parallel—a result which confirmed the previous observation that neither the mean difference in temperature between cyclonic and anticyclonic days, nor the relative frequency of their occurrence, could give rise to the second-order variation in atmospheric temperature, or indeed to any other simple periodic effect. It was therefore decided to abandon the separate classification of cyclonic and anticyclonic days.

Examination of the curves above described showed that the number of days on which any particular wind occurred during a ten-day interval in five successive years, was not sufficient to eliminate the effect of exceptional days from the mean value obtained. A further period, from 1880-1884, was therefore examined, and the curves were constructed from monthly means, instead of from ten-day means, for the nine years. Thus sufficient observations were obtained to give mean values which might be regarded as independent of single exceptional variations from the mean. The observations which were retained for the investigation were those relating to the days which had been previously classed as cyclonic or anticyclonic; the days which were not classified, on account of the want of any definite indication, were omitted. That this omission did not appreciably affect the results arrived at, may be inferred from the fact that one of the curves (Diagram 6, fig. 1, p. 75) could be obtained in two ways—first, by taking the temperature difference and frequency of wind for the included days, and, secondly, by taking the temperature difference from the actual means for all days. The results were quite identical in the two cases.

Effect of Wind-direction on Atmospheric Temperature during the Nine Years 1876-1884.

The effect of wind-direction on temperature was investigated by means of curves whose co-ordinates are respectively proportional to

the period of the year and to the difference on any day between observed mean daily temperature for the nine years and the temperature which would be represented on that day by the first order curve obtained by analysis of the twenty-five-year curve. The curves so obtained will be spoken of as the temperature curves each wind; but it must be remembered that they do not represent observed temperatures, but differences between mean observed temperatures and temperatures given by the first-order curve at given period of the year.

The term "for each wind" also requires some amplification. Owing to the marked effect of local geographical conditions at station, the recorded direction of the wind does not necessarily give a very accurate estimate of the true quarter from which the station obtains its air supply, and it is of course the source of air supply which will affect the atmospheric temperature at any station. The days were, therefore, not classified according to the direction of wind recorded as having been observed at Kew, but according to direction of the gradient (high to low) at right angles to the isotherms in the neighbourhood of the station, and a curve has been called the east wind curve when it really represents a curve for a southern gradient, and so on for other directions. It must be observed that the term "east" is not quite accurate, since the direction of supply for a southern gradient will not be from due east, but from a little north of east. However, as we are only dealing with the points of the compass, the direction of the wind when the gradient is to the south will be described with sufficient accuracy as an east wind.

In Diagram 3 the temperature curves for each wind are given. It will be seen that each curve tends to resemble in some important respects and in kind, if not in degree, the second-order curve for the twenty-five-year means. For example, the east wind temperature curve has a minimum in the beginning of May, and maxima in August and February; but these are not equal minima and maxima, and the curve is obviously not simply harmonic. The north-east wind has a minimum in spring and autumn and a maximum in winter and summer although the dates do not exactly correspond with those of the wind. The curve for the north wind is irregular but has a distinct absolute maximum at the end of July. The curve for the north-west wind has its maxima in February and at the end of July, and minima in spring and autumn, and so on. Thus all the winds have curves whose characteristics suggest those of the second-order curve, although they are not identical with them, and the effect represented by the second-order curve is still apparent in the temperature curves for each separate direction of air supply, and consequently is to some extent independent of the quarter from which the air is supplied.

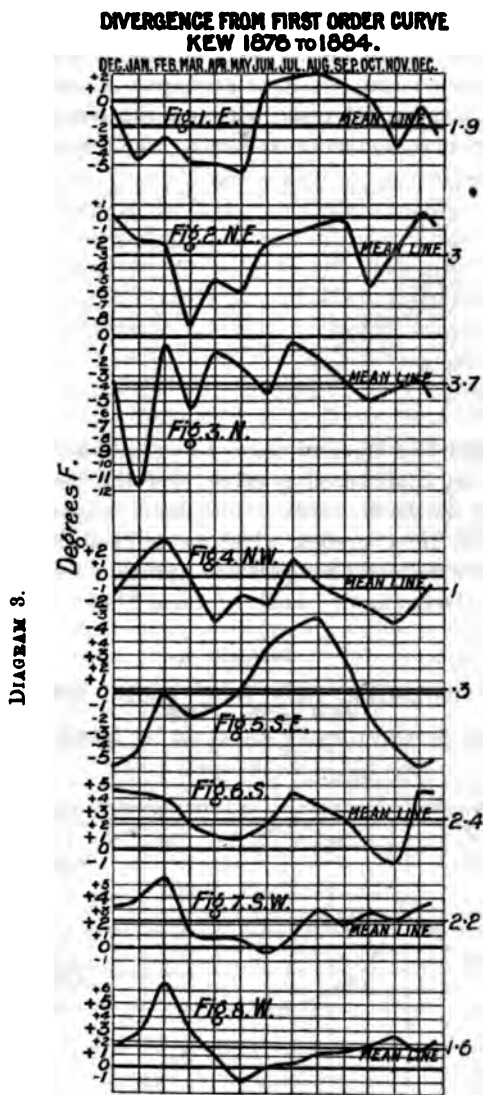


DIAGRAM 8.

FIG. 1. During prevalence of E. winds.

2.	"	"	NE.
3.	"	"	N.
4.	"	"	N.W.
5.	"	"	S.E.
6.	"	"	S.
7.	"	"	S.W.
8.	"	"	W.

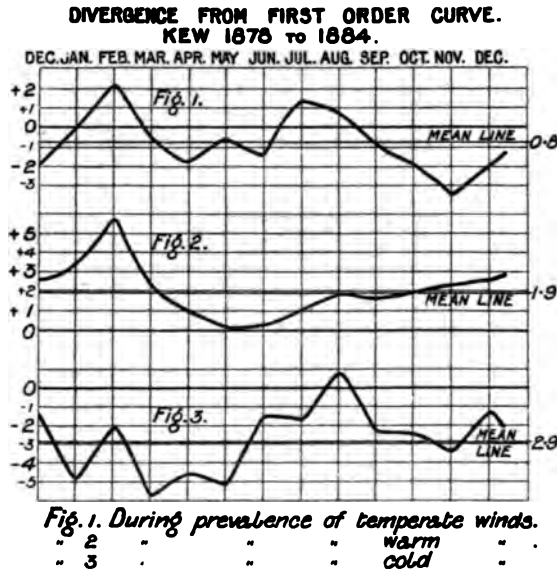
Warm, Cold, and Temperate Winds.

This has been shown more clearly by grouping the winds according to their mean effect on atmospheric temperature. It was found that the mean difference between the observed temperature and first-order curve temperatures for the whole year for—

1.	• E.	wind (as defined above)	was	- 3°·1 F.
2.	N.-E.	"	"	- 4·0
3.	N.	"	"	- 3·5
4.	S.-E.	"	"	- 0·6
5.	N.-W.	"	"	- 0·7
6.	W.	"	"	+ 1·9
7.	S.-W.	"	"	+ 2·2
8.	S.	"	"	+ 2·5

Seeing that the first three of these had a decided cooling effect, the next two a very slight cooling effect, and the last three a warming effect on the first order atmospheric temperature, they were studied in three groups, which we will call the "cold," "rate," and "warm," and temperature curves were drawn for these groups (Diagram 4). Each of these three curves sho

DIAGRAM 4.



resemblance with the second-order curve. There are very winter maxima in February in all three. Both cold and war

have minima in May and maxima late in July or early in August, and the temperate winds have a very distinct minimum in November. It is obvious, therefore, that the second-order effect is present in each separate wind and in separate groups of winds. Nevertheless we have eliminated, by separating the effect for each wind, part of the physical cause which gives rise to the second-order effect.

This is again shown in the curve which is obtained by compounding the eight temperature curves (Diagram 6, fig. 2, p. 75). This gives us a curve whose difference from the second-order curve* will represent that part of the effect which has been eliminated by separating the weather into these eight types: any effect on temperature which may arise from the relative frequency with which these various types occur will not have been taken into account. It will be seen that the characteristics of this curve (fig. 2, Diagram 6) approach those of the second-order curve far more closely than do those of the curves of individual winds. It has distinct maxima in February and August and minima in April and November, but the curve is not regular in its period although the period is approximately semi-annual. The February maximum follows a very sudden rise, and lies between two values for January and March which differ but very little from the minimum values in April and May; and moreover the minimum occurs somewhat earlier than the minimum of the second-order curve.

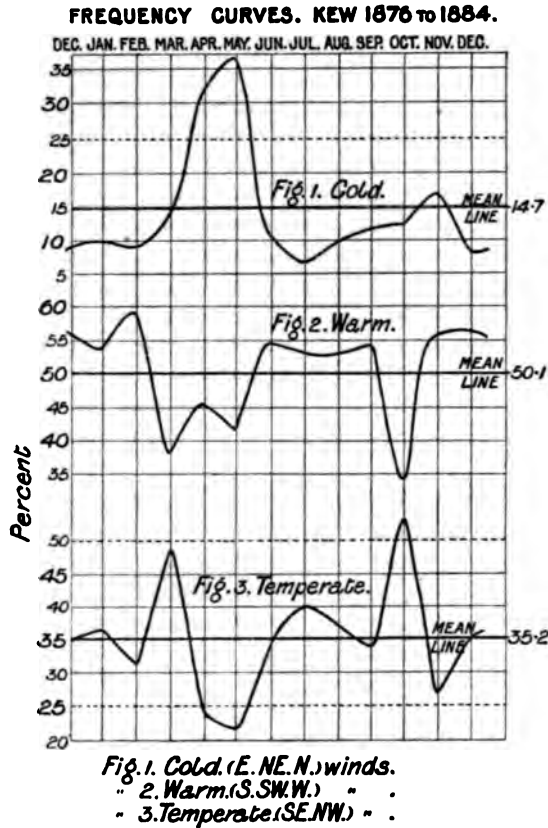
Relative Frequency of Occurrence of the Warm, Temperate, and Cold Winds.

We have thus separated a part of the physical cause of the second-order effect—a part which depends on the direction of air supply, i.e., on the quarter from which the air is derived. The analysis may be pushed further by means of the three groups of winds which we mentioned above. Curves were drawn for each group of winds whose co-ordinates are respectively proportional to the period of the year and to the percentage of the total number of days (in each month for the nine years) on which the winds belonging to that group were observed to prevail (Diagram 5, p. 74). It will be seen that the cold group shows a very marked maximum percentage in May and a less marked one in November, and minima in July and at the end of December, all of which characteristics correspond approximately with the second-order curve. The curve for the warm winds shows an inverse correspondence, while the curve for the temperate winds, whose mean effect is small, but whose tendency is on the whole to lower the temperature, has certain points of correspondence with the curve for cold winds. It is obviously impossible to represent the total share of this effect of wind

* Fig. 1, Diagram 6, represents the total residual temperature after subtracting the twenty-fifth year first-order component, and therefore shows the second component combined with the residual annual component for nine years, which has an amplitude of nearly 5° F. and components of shorter period.

direction in bringing about the second-order effect by any direct composition of these curves, but the fact that each of groups gives a curve of frequency whose characteristics remarkable correspondence with those of the second-order sufficient evidence that the relative frequency of occurrence

DIAGRAM 5.



from the various quarters plays some part in causing the se effect. What this part is is shown in Diagram 6. Fig. 1 curve of mean temperature difference throughout the year, gives the curve which is directly compounded of all the te curves for the various winds, so that the effect of relative fr eliminated ; fig. 3 is obtained by subtracting the ordinates of the corresponding ones of fig. 1, and may be taken as repres effect upon temperature arising from the relative frequency c cold winds. The main features of this curve are the winter

and spring minimum, and a glance at figs. 1 and 2 will show that it is the frequency which converts the sudden warm effect in February, which is shown by the individual winds, into a gradual rise from autumn to winter and fall from winter to spring which are the features of the complete second-order curve. It will also be observed that the frequency effect has a maximum in July which brings back the maximum exhibited by the sum of the individual effects to its normal

DIAGRAM 6.

DIVERGENCE FROM FIRST ORDER CURVE.
KEW 1876 TO 1884.

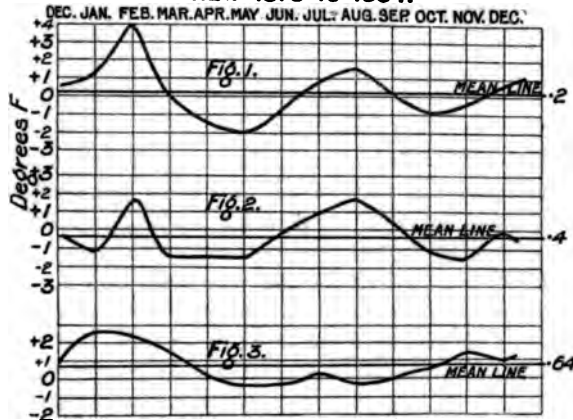


Fig. 1 Total effect.

- " 2. Effect of compounding the curves for all winds without regard to the relative frequency of the various winds or groups of winds.
- " 3. Difference between Fig. 1. and Fig. 2.

position. The October—November effect seems to be complete in the compounded curve, and, as was seen in Diagram 4, it has its origin chiefly in the curves of the south-east and north-west winds. Thus it appears that the second-order effect is to some extent independent of the direction of air supply, but that it is in part due to the relative frequency at different times of the year with which the air supply is derived from "hot," "cold," or "temperate" sources. At the periods of the maxima of the second-order curve the air supply of these islands is derived mainly from the warm sources, *i.e.*, south, south-west, and west, and at the periods of its minima, either from the coldest sources, which are the east, north-east, and north, or to a very predominant extent from the slightly cold sources, which are the south-east and north-west.

It is especially noteworthy that the cooling effect in May is due to

the relative prevalence of winds from cold quarters (see Diagram fig. 1), combined with the fact that "warm" winds and "cold" (Diagram 4, figs. 2 and 3) are relatively colder at that time than other times of the year; whereas the cooling effect in October and November may be attributed to the prevalence of the winds we called "temperate" (i.e., N.W. and S.E. winds), which at that time of the year become especially cold.

The July maximum corresponds to exceptional warmth of generally cold or temperate groups of winds (figs. 1 and Diagram 4), and the January maximum corresponds especially to frequency of occurrence of warm winds.

Effect of the Length of Period Analysed on the Position and Amplitude of the Curve.

As has been stated above, these effects have all been studied at Kew, and reference has always been made to the analysis of the twenty-five-year mean curve of temperature for Kew. It will, however, be seen that a possibility of error is here introduced, since it can be assumed that the mean curve for the nine years on which our analyses have been mainly based would have the same harmonic coefficients as the curve for the twenty-five years. Analyses were therefore made of the curves of mean temperature for various periods of years as given in Table I. It will be seen that the first-order curves for the ten, five years and nine years are practically identical, and the positions of the maxima of both the first- and second-order curves are also practically identical for the two periods of years. The third- and fourth-order curves are larger for the shorter period, but are still relatively small so that the curves of difference from the twenty-five-year first-order curve, which we have been discussing above, fairly represent (as has been assumed) the nine-years second-order curve.

It may here be of interest to make a further study of Table I. It will be seen that not only the twenty-five and nine-year curves have been analysed, but also the curves for the five years and four years which made up the nine years, and again for four single individual years. Of these individual years, the year 1884 was chosen at random, the year 1873 on account of its extremely warm winter, the year 1895 on account of its extremely cold and late winter, and the results of the analysis for 1898—a year with exceptionally hot weather in August and September—have been added to the table. It will be seen that a retarded first-order curve, with a second-order curve of small amplitude and rather early maximum. It will be noticed that the coefficients of the first-order curve are practically the same in all the analyses, except those of the very abnormal years. A still more striking phenomenon is the persistence of the second-order

not only in the twenty-five years, the nine years, the five years, and the four years, but also, and to an exaggerated extent, in the typical single year. The higher-order effects vary very much both in magnitude and in position, but the second-order effect is prominent in all the curves, and, except in the very exceptional years whose abnormality it shares, it has its maxima oscillating over the first ten days of February and August respectively. This oscillation again shows itself in the gradual decrease of the amplitude of the second-order effect as the number of years increases whose mean curve is analysed. Thus for the single year the amplitude is $3^{\circ}57$ F.; for the five years it is $2^{\circ}06$ F.; for the nine years $1^{\circ}95$ F.; and for the twenty-five years, $1^{\circ}38$ F. Thus Table I is in itself a sufficiently striking demonstration of the existence of a second-order meteorological effect which combines with the primary solar effect to give us our normal periodic variation of atmospheric temperature.

The gradual diminution of the amplitude of the second-order oscillation with the extension of the time over which the means are calculated, might perhaps be taken to indicate that if the means were sufficiently extended it would disappear altogether; and the fact that in hundred-year means for Vienna (which will be referred to later), the second-order amplitude is reduced to one degree, whereas the first-order amplitude is $19^{\circ}8$ F., might be regarded as a further indication of the same fact. In the absence of data extending over the whole period it is, of course, impossible to form a definite opinion, but if the arrangement were so that the effect which appears in twenty-five-year means is obliterated in a hundred years, it would be very interesting to know how the second-order effect appeared in the other three groups of twenty-five years of which the hundred are made up. Some light might be thrown upon the matter by the analysis of all the component years of the original twenty-five, but this has not yet been carried out.

The fact that the maximum of the second order for Vienna comes on March 24 shows that the remaining effect there is different in character from that shown in the curves for the British stations.

Comparison of the Effect at Kew with that at the other Stations.

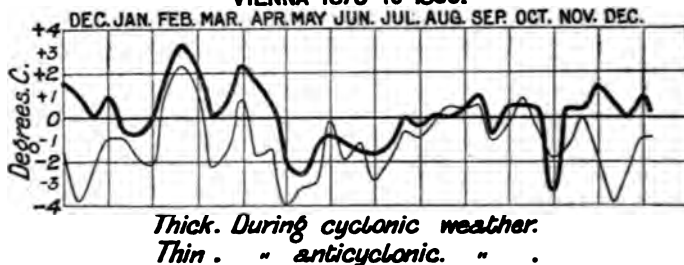
Reference has hitherto only been made to the Kew curves, since it is the Kew temperatures which have been studied in detail, but the analysis of the twenty-five-year mean temperature curves for Aberdeen, Valencia, and Falmouth are also given in Table I. The second-order curve is in each case about one-eighth of the first-order curve in amplitude, but the third-order curve is not always entirely negligible. At Kew its amplitude is only one-thirtieth of the second-order curve amplitude, and at Falmouth it is only one-seventh, so that in these

cases the higher orders are entirely negligible. At Valencia and Aberdeen, however, the amplitude of the third-order curve approaches one-third of that of the second-order curve, and therefore becomes more important; but its amplitude amounts to only one-third of a degree, and the second-order effect thus remains the only one of any practical importance.

Mr. A. A. Rambaut* has harmonically analysed the curve of the monthly mean temperatures for a single year (1899) at Oxford with similar result, and in this case, as in the analysis of the 1884 Kew temperatures, the second-order curve is comparatively very large. It will be seen that all the stations hitherto mentioned have been on or near the sea-coast, and indeed it may be said that all British stations are comparatively near to a sea-coast. It was therefore thought advisable to make an investigation of the temperatures at a Continental station, where the effects due to the proximity of the ocean would be eliminated. For this purpose Vienna was chosen. Dr. Hann, in a paper on the "Temperature of Vienna," based on observations for 100 years, gives the harmonic analysis of the curve of mean daily temperatures (Table I, Vienna). The amplitude of the second-order curve is only $\frac{1}{10}$ th of that of the first-order curve, and that of the third-order curve is approximately of the same magnitude. Moreover the weather at Vienna has been investigated for the years 1876-1888 in the same way as the weather at Kew, and curves of temperature difference from the first order curve value have been drawn for each wind and each type of weather. No definite periodic curves were obtained similar to those obtained for Kew, all the curves being different from one another, and in themselves irregular: the only universal characteristic was that the cyclonic and anticyclonic curves for the

DIAGRAM 7.

DIVERGENCE FROM FIRST ORDER CURVE.
VIENNA 1876 to 1880.



whole year (Diagram 7), and moreover for each wind, showed a remarkable parallelism, the cyclonic being almost always a little warmer

* 'Roy. Soc. Proc.,' vol. 67, p. 221.

than the anticyclonic days. Thus there is no prominent effect at Vienna analogous to our second-order effect, but, as in these islands, whatever periodic meteorological temperature effects do exist, appear to be entirely independent of the relative frequency and mean temperature of cyclonic and anticyclonic weather. An analysis of the temperature curve for Agra, a typical Continental station in India, has also been made (Table I, Agra). Here there is a conspicuous second-order curve, but its position is entirely different from that of the corresponding curve for the British Islands, and its effect is to prolong the summer, whilst the second-order effect which has been discussed above shortens it. The two effects are thus in no way analogous, and this result, combined with the result of the Vienna investigation, makes it seem probable that the ocean plays a paramount part in the causation of the second-order temperature effect which we experience in these islands.

Analysis of Sea Temperatures.

With the object of further investigating this point, curves whose ordinates are respectively proportional to the fraction of the year, and to the mean temperature of the sea, based on three years' observations at Shetland, Scilly, and Yarmouth, have been analysed (Table II). The maxima and minima of these curves are all a little later in the year than those of the air-temperature curves, but the lag is approximately the same in all of them, and it is an obvious and striking fact that there is in the periodic variation of marine temperature an effect similar to the second-order effect observed in the periodic variations of atmospheric temperature. Whether this variation of the temperature of the water which surrounds these islands is the cause of the atmospheric second-order variation, or whether it is only another effect of the same fundamental cause, does not appear; but in view of the fact that the marked second-order effect is not seen at Continental stations, it would seem not unlikely that the ocean temperature is the immediate cause of our second order periodic temperature variation.

Analysis of Barometric Differences between London and Valencia and London and Aberdeen.

Table III gives the results of the analysis of the curves of thirty-year-mean difference of barometric pressure between London and Valencia, and London and Aberdeen, respectively. In each of these curves we again find the prominent second-order effect, although in the curve for London—Valencia it is somewhat earlier in its position than the temperature effect. Thus the magnitude of the barometric gradients for southerly and easterly winds shows a similar second order periodic variation to that which has been observed in the atmospheric temperature of these islands. In view of the close

connection between barometric pressure and atmospheric temperature it seems not unlikely that the one effect plays some part in the causation of the other.

The striking correspondence between the period of occurrence of maximum of the second-order component of the gradient for westerly winds (London—Aberdeen) and the corresponding components for temperature, leads naturally to the suggestion that the periods of maximum and minimum of the second-order component of temperature related to special barometric conditions. They may, and indeed may be connected with some stage in the process of annual migration of the centres of high and low barometric areas, which is the result of unequal distribution in longitude of the land areas in the north hemisphere. A cursory survey of the mean monthly distribution of pressures in Dr. Buchan's volume of 'Bartholomew's Atlas,' shows that April is an exceptional month of transition, but up to the present we have not found any means of representing the successive stages of the transition in such a way as to enable a comparison to be made between them and the second-order curve of temperature.

The data used in this paper have been compiled in the Meteorological Office, or extracted from the various publications in the library of the office. The harmonic analysis has also been carried out there under the supervision of Mr. R. H. Curtis.

Summary.

The harmonic analysis of the curve of mean atmospheric temperature at stations in the British Isles shows that there is, compounded with the primary solar variation, a secondary half-yearly meteorological variation.

The effect of this variation is to moderate and lengthen the winter and to intensify and shorten the summer.

The second-order curve which represents it, has maxima which occur late, in different years, over the first ten days of February and August respectively, and minima which oscillate over the first ten days of March and November.

With some exceptions, the effect is generally apparent in a single year's observations, and, owing to its varying position, its curve appears to have a larger amplitude in the analysis of the temperature curve of a single year than in that of the mean curve of a number of years.

It is independent of the relative frequency of occurrence of cyclonic and anticyclonic weather, and of the relative temperature of the air during the prevalence of these different types of weather.

It is partly due to a periodic variation in the relative frequency of occurrence of the "cold," "warm," and "temperate" winds, but

to some extent apparent during the prevalence of winds from all quarters.

It is not found, with maxima at the same epoch, in purely Continental stations, such as Vienna and Agra.

A similar effect is found in the temperature variation of the sea-water at stations surrounding these islands, and the atmospheric effect is probably connected with this.

A similar second order periodic effect is also found in the variation in the magnitude of the barometric gradient between London and Valencia and London and Aberdeen. It is probable that this periodic variation in pressure plays some part in causing the similar variation in temperature.

Note on the Effect of Sea Temperature upon the Seasonal Variation of Air Temperature.

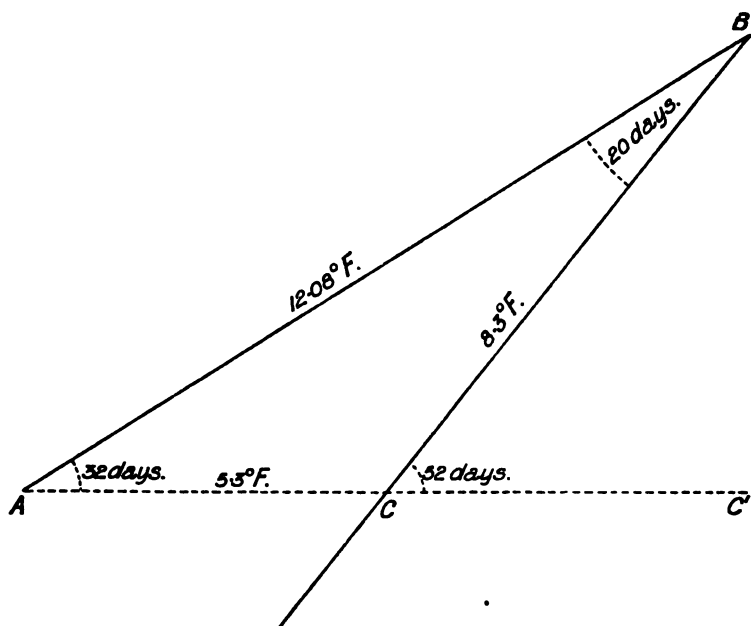
It has been pointed out in referring to Table II. that the harmonic components of the first and second orders of the seasonal variations of the temperature of the sea in the neighbourhood of a coast station show a relative lag as compared with the corresponding components of the air temperature of the stations. On comparing Table II with Table I, it will readily be seen that the first-order curves for the coast stations show a definite lag as compared with the inland and Continental stations. Thus the earliest occurrence of a first-order maximum for a coast station in Table II is July 30, and in Table I July 28, while the relatively inland station, Kew, shows the first-order maximum on July 23, and Oxford (for a single year, however) on July 12. Vienna is earlier than Kew, July 18, and Agra still earlier, June 29. We may thus regard the sea as causing a considerable lag in the occurrence of the seasonal variations of temperature in the adjoining coast stations, while in the sea itself the lag is still greater.

Now we may regard the periodic variation of sea temperature as a cause affecting the air temperature of the stations, and, without knowing precisely the process connecting the cause and its effect, we may form some estimate of the relative magnitude in the following manner. Dealing only with the first-order components of the two related phenomena, we note that they are harmonic variations of the same period. We may assume that the effect of the periodic cause is itself simply harmonic and of the same period, and that the resultant first-order component for the air temperature is made up of a part which is independent of the sea temperature and a part which is dependent upon and caused by the sea temperature. We may thus assume the resultant first-order curve to be made up by compounding the two separate component curves.

The composition of harmonic curves of the same period is a geo-

metrical process. Thus, if the length of AB, in fig. i, represents the amplitude of the resultant curve, AC, CB, the amplitudes of the two components, then, upon forming a triangle of the three, as in the figure, the angle BCC will represent the phase difference of the two components, BAC will represent the phase difference between the resultant and the first component, and ABC the phase difference between the resultant and second component.

FIG. i.



Applying these to the case under consideration, AC may be regarded as representing the temperature oscillation undisturbed by the effect of the sea temperature, CB the effect of the sea and AB the resultant effect or the actual temperature oscillation observed at the station. The angle ABC will represent the number of degrees of phase difference between the sea curve and the station curve, *i.e.*, practically, the number of days between the time of maximum on shore and on sea, and so on for the other angles. Of such a triangle for any station, the following elements are known: the resultant amplitude AB, the lag of the sea effect behind the land curve, *i.e.*, the angle ABC. The position of C is indeterminate because we do not know the actual magnitude of the sea effect BC, nor the magnitude or epoch of the undisturbed effect. But we know the extreme position of AC: it cannot correspond to a curve with an earlier maximum than June 21,

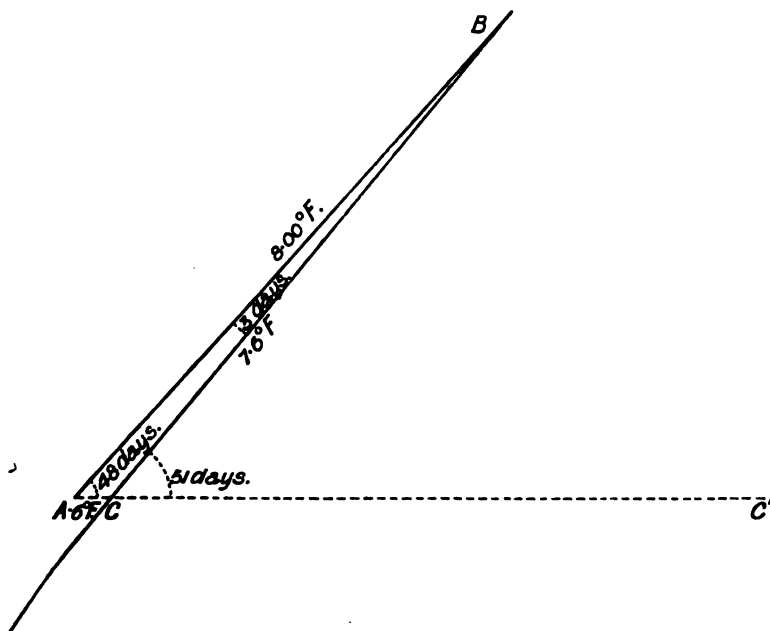
when the planetary effect reaches its maximum. Drawing then the line AC corresponding to a maximum on June 21, we know that the sea effect cannot exceed BC in magnitude, and that the effect which is independent of the sea, cannot be less than AC.

Fig. i is a scale drawing representing this analysis of the temperature oscillation for Kew into two parts. It will be noticed in Table II that the sea temperature reaches its maximum at the Shetlands, at Scilly, and at Yarmouth on from August 10 to 13. Taking August 12 as a mean date, the sea effect reaches its maximum 20 days after the maximum of the first-order curve at Kew; the line BC is therefore ruled at 20° to AB. AC, being unknown, is dotted at an angle of 32° , corresponding to 32 days, the lag of the time of maximum at Kew behind the solstice (June 21). It follows from the measurements of the diagram that the sea effect at Kew cannot exceed $8^\circ\cdot3$ F., and the original effect, apart from sea, cannot be less than $5^\circ\cdot3$ F.

The mean amplitude of the sea variation is nearly 8° F., so that if the lag of the seasons at Kew is wholly due to the sea, the whole variation of temperature of the sea is superposed upon the initial variation. This would no doubt be an exaggeration, and the initial variation at Kew must be greater than the $5^\circ\cdot3$ F. shown in the diagram.

A corresponding diagram drawn for Scilly from the air and sea temperatures there (fig. ii) represents nearly the whole temperature

FIG. ii.



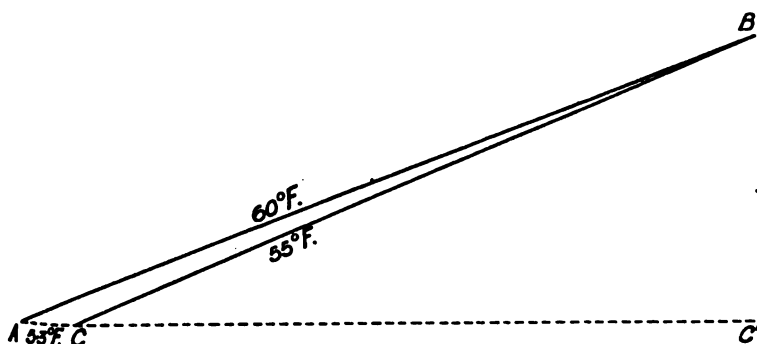
variation at Scilly as being due to the variation of sea temperature but the sea temperatures are taken from means for a very short period these being the only ones immediately available, and it would require a much more prolonged investigation of the sea data before a trustworthy diagram could be drawn. When these are obtained from a number of stations it would not be difficult, by assuming the unturbed climate to be the same for places in the same latitude, to conduct a satisfactory analysis of the elements of temperature of a place into its primary constituents. It is, however, apparent from the mode of resolution of the temperature oscillation into components that the more the components approach to coincidence of epoch the greater is the resultant effect. For example, if the sea temperature round the British Isles reached its maximum on June 21, the amplitude of the temperature at Kew would be raised to $13^{\circ}6$ F., and accordingly part of the intensity of a Continental climate may be attributed to the simultaneous heating of the large area over which the air for a Continental station passes.

There is another point of some interest which arises in connection with this subject, and that is, that all the successive stages of temperature change are delayed by the effect of the sea. Since both first- and second-order components of the variation of sea temperature are of the same order of magnitude as those of the land curves, the effect of the sea is to delay the seasons, and we should on that account expect, not only the autumn and winter, but also the spring, to be later in a marine climate like that of Scilly or Falmouth than in an inland climate, and it would be interesting to ascertain how far such a conclusion is borne out by phenological observations. If this conclusion should be borne out, and there is at least some evidence in favour of it, one should seek an early spring, not on the coast, but inland, and on the other hand a late summer is more characteristic of the coast than of the inland country.

It may seem surprising that the effect of the sea upon the annual temperature oscillation of a place near it should be regarded as increasing the amplitude of the oscillation, whereas it is a matter of common knowledge that the effect of sea upon climate is to reduce the amplitude of the temperature oscillation as compared with that at a Continental station. But in fact the temperature range of any place depends upon the original range (which may be regarded as depending merely upon its latitude) and the increase of its range due to its surroundings. In the case of an ocean station the increase of range is small, because the temperature range of the ocean is itself small, and the epoch of its maximum is two months later than that of the planetary effect. At a Continental station the increase due to the surroundings is much larger and more nearly simultaneous with the original planetary effect.

The climatic conditions at a place like Kew, for example, which is not far from the sea, and which may be regarded (according to fig. 1) as having an original planetary temperature range of $10^{\circ}6$ F., and a resultant range of 24° F., may be contrasted with those for such a station as Nertchinski-Zavod,* at about the same latitude, with a range of over 120° F. Its maximum is reached by the middle of July, consequently the composition of the original planetary range with the range due to the surrounding region is almost algebraical. On that understanding, and assuming that the planetary range is the same as that at Kew, we find that the influence of the surrounding land at Nertchinski-Zavod is to increase the range by 110° F., whereas the effect of the sea at Kew only adds $13^{\circ}6$ F. to the range, *i.e.*, less than one-eighth of the land effect on the same latitude. The corresponding diagram would be as represented in fig. iii upon a scale of temperatures one-third of that of the other figures.

FIG. iii.



It is not, of course, a legitimate assumption that the planetary range corresponding to the latitude of Kew is accurately represented by $10^{\circ}6$ F., which requires only to be compounded with the sea effect to produce the resultant oscillation. The resultant effect at Kew is probably resolvable into three components—one the original planetary effect with its maximum in June, a second due to the surrounding land with its maximum in July, and the third due to the sea with its maximum in August. The precise epoch of maximum of the land effect, which would identify the direction of its line on the diagram, and the actual magnitude of any one of the component effects, which would fix the position of the lines in the diagram, are not at present determinate, but their extreme limits are known.

* *Atlas Climatologique de l'Empire de Russie.* St. Petersburg, 1900.

"Some Physical Properties of Nitric Acid Solutions." By V. H. VELEY, F.R.S., and J. J. MANLEY, Daubený Curator, Magdalen College, Oxford. Received February 11,—Read March 7, 1901.

SECTION I.—*Introductory.*

The results obtained in our former communication on the "Electric Conductivity* of Solutions of Nitric Acid" have led us to continue our investigations upon other physical properties of the same substance, partly with a view of confirming our conclusions, and partly also because previous observations on firstly, the refractive indices of samples of varied concentration, and secondly, the contractions on admixture of the acid with water, are comparatively few.

The conclusions, at which we previously arrived, were that the values of the electric conductivities to a less degree, and the electric temperature coefficients to a greater degree, varied at percentage points approximately corresponding to the composition required by the hydrates $\text{HNO}_3 \cdot 3\text{H}_2\text{O}$, $\text{HNO}_3 \cdot 2\text{H}_2\text{O}$, $\text{HNO}_3 \cdot \text{H}_2\text{O}$, and $2\text{HNO}_3 \cdot \text{H}_2\text{O}$; the two latter being the analogues of orthophosphoric H_3PO_4 and pyrophosphoric acid $\text{H}_4\text{P}_2\text{O}_7$ respectively.

It was further shown that acid of 96—99·7 per cent. concentration showed remarkable peculiarities, and the suggestion was put forward by others and ourselves that an initial decomposition into water and nitric anhydride takes place.

In the present work determinations were at first made both of the refractive indices and densities for percentage differences of approximately 5 per cent.; a rough curve was then drawn, and from any indications given of marked variations of curvature, observations were more especially multiplied on either side of points which might be regarded as critical.

As regards the division of labour, the former of us is mainly responsible for the contraction and analytical determinations, the latter for the optical measurements and the apparatus connected therewith.

The work was conducted partly in the Chemical Department of the University Museum and partly in the Laboratory of Magdalen College. Lastly we desire to express our thanks to the Government Grant Committee of the Royal Society for apparatus purchased or lent; to the Chemical Department of the University Museum for loan of instruments; to Mr. E. H. Hayes, of New College, for assistance in verifying and correcting calculations.

* 'Phil. Trans.,' A, 191 (1898), 365.

II.—Arguments of Former Writers.

Mendeleef * from results, the source of which is not quoted, has put forward three equations to represent the densities d in terms of percentage p as follows :—

From 0 to 41·2 per cent. ($\text{HNO}_3 \cdot 5\text{H}_2\text{O}$):

$$d = 9992 + 57 \cdot 4p + 0 \cdot 16p^2.$$

From 41·2 to 63·63 per cent. ($\text{HNO}_3 \cdot 2\text{H}_2\text{O}$):

$$d = 9570 + 84 \cdot 18p - 0 \cdot 24p^2.$$

From 63·63 to 100 per cent. :

$$d = 10652 + 62 \cdot 08p - 0 \cdot 16p^2.$$

(Density of water at $4^\circ = 10000_{\text{t}}$)

It was found, however, that the differences between the observed and calculated values were greatly in excess of those which could be accounted for by errors of observation. Pickering,† by the application of curves to the results of Berthelot‡ for the heats of the dissolution and of Kolb for the specific gravities, deduced the existence of and isolated in a crystalline form the two hydrates $\text{HNO}_3 \cdot 3\text{H}_2\text{O}$ and $\text{HNO}_3 \cdot \text{H}_2\text{O}$. He also alludes to a minor alteration of properties at 18–20 per cent., but without drawing any conclusions therefrom, and to the fact that the maximum contraction coincides with the composition of a definite hydrate.

III.—Purification of Materials and Preparation of Stock Samples.

The methods of purification have been fully described in our previous communications, and our experience has as yet pointed to no improvement, though we have had occasion from time to time to confirm the observations of others, working at other times and places, as to the persistent retention of some ammoniacal compound in ordinary distilled water.

Continued experience has shown that by the methods adopted for the purification of nitric acid, the amount of nitrous, sulphuric, and halogen acids did not exceed 1, 4, and 3 parts per million respectively, quantities which may fairly be presumed to have no effect upon the physical constants.

* 'Principles of Chemistry,' Eng. Trans. (2nd ed.), vol. 1, p. 272.

† 'Journal of the Chemical Society,' 1893 (Trans.), pp. 436–443.

‡ 'Chimie Mécanique,' 1, p. 397.

IV.—*Determination of the Concentration of the Samples.*

It is believed that in the majority of cases the mean error does greatly exceed one part per thousand. In every case the determinations of the concentration of the samples were conducted either immediately before or after those of the densities or refractive indices, as the concentrated acid is as hygroscopic a substance as sulphuric acid.

In order to show the degree of concordance obtainable from various methods the following cases may be cited.

The value of a certain soda solution was determined at constant temperature as against a certain hydrochloric-acid solution; variable quantities five values were obtained with a mean error of part in 2844 parts, calculated by the method of least squares. hydrochloric acid solution had previously been standardised on two different occasions; at one time as against recrystallised silver nitrate purified sodium carbonate, and at the other against another sample silver nitrate of presumably different past history; the mean error by the one method was ± 1 part in 1850 parts, and the total mean by the two methods was ± 1 part in 1298 parts.

The same solution was determined twice as against a certain sulphuric acid solution, the value of which was ascertained by determining its density at $15^{\circ}/4^{\circ}$, ascertaining its percentage strength by interpolation from Pickering's tables, and hence the amount of acid in 1 c.c.

The results of all the determinations may be summarised as follows and expressed as nitric acid equivalent to soda in 1 c.c.

(i.) By hydrochloric acid method	=	0.06136	gramme
(ii.) By sulphuric	"	0.06144	"
Mean.....		0.06140	"

Namely, a mean error of ± 1 part in 1533 parts.

By soda it is desired to imply alkalinity only; the metallic sodium used for its preparation might have and probably did contain quantities of other alkaline metals, but as such soda solutions may be used as go-betweens to express the one acid in terms of the other the possible effect of such impurities is thereby eliminated.

Attention has recently been drawn to errors in burettes, not on the Reichsanstalt of Berlin but also by individual writers, especially Wagner;* these errors may be summed up as those of (i) Parallax, (ii) temperature, and (iii) drainage. The last of these varies with the length drained, the viscosity of the solution, and often wholly unknown condition of the cleanliness of the glass.

* 'Zeits. Physikal. Chem.,' vol. 23 (1899), pp. 193-219.

The cause of greasiness in burettes, a perpetual source of trouble in accurate work, has been discussed by various writers, and attributed by them to oils or fats in the distilled water derived from the lubricating materials of the still (Ostwald), or to the vaseline, &c., used for lubricating the stop-cock. Though probably both causes may be at work, yet we consider that this greasiness is produced also by slime-forming bacteria or hypomycetes, especially *Dematium*, the spores of which settle and develop on the sides of the vessel; our experience has shown that if the air sucked in by the outflowing solution be filtered through cotton-wool plugs, and if the burette be filled invariably from below, and not, as is customary, from above, its state of cleanliness may be greatly prolonged.

The first table of specific gravities of nitric acid in terms of percentages was constructed by Kirwan;* subsequent tables were published by Ure,† Thenard and Kolb.‡ The numbers given by the last are still reproduced more or less fully in text-books of chemistry and physics, though it is now generally admitted that his results for the more concentrated acids are too low.

But as a considerable advance over previous writers, Kolb pointed out, firstly, that the presence of the lower oxides of nitrogen alters appreciably the value of the density; secondly, that acid of approximately 100 per cent. concentration has a high coefficient of expansion; and thirdly, that the maximum contraction on admixture with water occurs at the percentage composition required for the hydrate $\text{HNO}_3 \cdot 3\text{H}_2\text{O}$. Upon the last result Kolb expressed himself as follows: "Ce fait d'un maximum de contraction correspondant exactement à un mélange en proportions définies n'est pas un effet du hasard." The most recent work upon the densities is that published by Lunge and Rey,§ which is now more generally quoted in technical manuals.

The analytical methods of these last writers are similar in outline to those herein adopted, but their final process of purification consisted in distilling the concentrated acid with sulphuric acid *in vacuo*, a method which might cause slight contamination, as the vapour pressure of the latter acid under these conditions could not be an insignificant quantity.

The densities were determined for $15^\circ/4^\circ$, but it is not evident as to those actually made, and those interpolated in order to reduce the values to the commercial scale of Twaddell; further, the results are only taken to 0.001, which is far within the limits of accuracy attainable.

These writers, in conjunction with Marchlewski, have given a table

* 'Irish Acad. Trans.,' vol. 4, p. 3 (1790).

† 'Quart. Journ. Sci.,' vol. 4, p. 291 (1818).

‡ 'Ann. Chim. Phys.,' [4], vol. 10, p. 140.

§ 'Zeits. f. Angew. Chemie,' 1891, p. 167, and 1892, p. 10.

of corrections for (i) percentage proportion of nitrogen peroxide, and (ii) variations of temperature.

These writers further pointed out that if the values for the densities of acids above 70 per cent. be represented in terms of percentages, such values lie upon a curve and not a straight line, as indicated by the determinations of Kolb. Various isolated determinations of acids of various strength have been published by Kohlrausch and Perkin for special purposes in the course of investigations on electric conductivity and specific magnetic rotatory power.

V.—*The Sources of Error in Pyknometers.*

The sources of error may be classified under three categories, namely, (i) weight of glass and its contents taken by themselves; (ii) temperature; and (iii) adjustment.

Firstly. *Weight of Glass and Contents.*—These arise from (i) *corrosion*, and (ii) *absorption*.

To avoid the second error of absorption the acids were taken in either a gradually ascending or descending series of concentration; between any set of observations the tube was not washed out with water, but rinsed frequently with the acid to be subsequently used. The object of this precaution is to gradually season the vessel, as it were, for the particular acid to be examined, as undoubtedly some of the acid is soaked into the glass; if, then, in a subsequent observation the acid formerly absorbed is soaked out and replaced by an acid of different concentration, the error produced in the weight of glass and acid is minimised. The methods recommended by various writers of cleansing with water, steam, or dry air were found to cause error, while the use of alcohol and ether, formerly in vogue, was quite inadmissible.

Secondly. *Adjustment.*—The instruments were placed in a water-bath at the required temperature, and the solutions drawn off from one limb by bibulous paper until the level-mark on the opposite limb was attained; for the most concentrated acids, which at once nitrate and render useless the common form of bibulous paper, the only material found suitable was a kind of tissue paper, sold for a wholly different purpose.

In order to estimate the effect of an error purposely committed in the adjustment, the following method was adopted: One of the pyknometers filled with water was placed in a water tank, and a horizontal microscope with vertical movement was so arranged that the adjustment mark was in the field of view; on the eyepiece of the microscope was etched a 1 cm. scale, divided into hundredths, so that a difference of a deci-millimetre could be read off. As the adjustment mark appeared too coarse an object under the magnification, a brilliant

bubble in the glass itself, situated a few millimetres above the mark, was taken as a more convenient standard of reference; the distance between the lowest part of the bubble and the best defined limit of the adjustment mark was measured once for all. The water level was then adjusted so that the top of the meniscus was at this ascertained distance, and a weighing of the water contents made (temperature 24.2); in subsequent experiments the water level was purposely adjusted 1.6 mm. below and 1 mm. and 2 mm. above the standard mark, both series being required as the capillary limb was not a true cylinder.

The following results were obtained :—

No.	Level.	Water contents at 24.2 (corr.).	Difference.
I	0	48.9402 grammes	—
II	1.6 mm. (below)	48.9373 "	-0.0029 gramme
III	1.0 " (above)	48.9410 "	+0.0008 "
IV	2.0 " "	48.9428 "	+0.0016 "

These differences in the water contents would cause differences of 0.00008, -0.00002, and -0.00005 respectively in determinations of the acid of 44.1 per cent. concentration (*vide infra*), which would be either diminished or increased by one or two units in the case of acids of 100 per cent. and 10 per cent. concentration respectively.

Correction for Displacement.—All the weighings for the determinations were reduced to a vacuum according to the usual approximate formula

$$W = w \left(1 - \frac{\lambda}{\rho} + \frac{\lambda}{\sigma} \right),$$

wherein $\lambda = 0.0012$ is the adopted standard density of air under average conditions, and ρ taken as at 8.4; the circumstance that the smaller weights were of platinum instead of brass affects the results beyond the limits of experimental error and can therefore be neglected. The densities were throughout reduced in terms of the maximum density of water at 4° C.

Thirdly. *Temperature.*—The temperature was recorded by a thermometer, graduated to 0.1 C., and of such an open scale that 0.03 C. could be read without difficulty; the zero displacement and the stem errors of the instrument were determined in our previous investigation.

With a view of ascertaining if there existed any appreciable error in the thermometer (by Casella) used, and to examine the accuracy of the method, the density of a sample of 44.10 per cent. acid (selected as approximately the mean of water and 100 per cent. acid), was

determined by five different thermometers, namely, the one used for the investigation, one standard by Baudin graduated to 0.02 C., one by Delaunay, and two by Becker, these last three graduated to 0.1 C. All these instruments were calibrated in our previous investigation and have subsequently been submitted again to the same process with the same results.

In each case two independent determinations of the water and acid contents were made, so that either four or two results were obtained according as all the determinations or one in each pair were different. The following results were obtained for the density at 24.2/4 :—

Thermometer.	I. Baudin.	II. Casella.	III. Delaunay.	IV. Becker (i).	V. Becker (ii).
Densities... {	1.26730 1.26731 —	1.26732 1.26734 —	1.26735 1.26734 1.26736 1.26737	1.26737 1.26732 1.26737 1.26734	1.26739 1.26730 1.26730 1.26732
Mean.....	1.26731	1.26733	1.26735	1.26735	1.26730

The mean of all the above determinations is 1.26733, with a probable error of ± 0.00002 calculated from the ordinary formula

$$0.674 \sqrt{\frac{\sum r^2}{n(n-1)}},$$

though it would appear probable from the results that the thermometers I, II, and V gave identical readings, but different from III and IV; this difference is probably equal to about 0.02 C. However this may be, the errors accumulated by the variations of one instrument would not affect any conclusions, and the method, as regards temperature, gives accurate results.

A number of duplicate experiments were made in which one or more of the following were varied: (i) stock acid; (ii) pyknometer; and (iii) standard alkali; the difference in the values obtained did not exceed one or two units in the fourth place of decimals, which would cause no appreciable error in the contractions.

Further, it is most probable that the divergence in values obtained for the contractions of acidic and saline solutions, to which attention has recently been drawn,* arises rather from errors of analysis and unequal amounts of impurities in materials, than from density determinations with all their consequent errors.

* Wade, 'Journ. Chem. Soc.', 1899 (Trans.), p. 254.

VI.—Contractions.

From the density determinations the contractions in the formation of 1 gramme of the acid solutions have been calculated. The volumes occupied by 1 gramme of water at the temperatures of 4, 14.2 and 24.2 are taken from Rosetti's tables as at 1, 1.00075 and 1.00268 c.c. respectively, while those of 100 per cent. acid are calculated with slight corrections from the density values obtained for the 99.97 per cent. acid previously prepared. Owing to the difficulty experienced in eliminating the last fractions per cent. of water from the acid no attempts were made to obtain another sample; therefore, if there had been an error in these density determinations the values for all the contractions would be uniformly increased or diminished, but the conclusions to be drawn therefrom not materially affected.

As it has been shown in the previous sections that from various causes differences in value occurred of some units in the fifth place of decimals in the density determinations, these figures were omitted as inconsequent for the purpose of calculation, and the results expressed in 1/100,000 c.c.

The results are calculated by means of the formula

$$c = pV/100 + (100 - p)V_1/100 - V_2,$$

wherein p = percentage of acid, V = volume occupied by 1 gramme of acid, V_1 = volume occupied by 1 gramme of water, and V_2 the reciprocal of the density for any particular mixture.

The details of one calculation are given for an example:

Percentage of acid = 61.53, of water = 38.47.

Volume occupied by 1 gramme of water at 14.2 C. = 1.00075 c.c.

Volume occupied by 1 gramme of 100 per cent. acid at 14.2 C.
= 0.65679 c.c.

Volume occupied by mixture $\left(= \frac{1}{D}\right)$ at 14.2 C. = 0.72383 c.c.

$$pV/100 = 0.40412 \text{ c.c.}$$

$$(100 - p)V_1/100 = 0.38499 \text{ c.c.}$$

$$\text{Sum} = 0.78911 \text{ c.c.}$$

$$V_2 = 1/D = 0.72383 \text{ c.c.}$$

$$\text{Contraction } C = 0.06528 \text{ c.c. or } 6528 \times 10^{-5}.$$

VII.—*The Graphical Representation.*

When the contractions are plotted out in terms of percentages, the former as ordinates and the latter as abscissæ upon a scale of 1 mm. for 10^{-4} c.c. and 10^{-1} percentage, there is in each case an ascending and a descending branch. The maximum corresponds to a percentage 54 per cent., a result which is in accordance with the former observation of Kolb, who found a constant maximum from 52.33 per cent. to 54 per cent. The value calculated from this observer's density at 15° for 53 per cent. (corrected to 14.2°) expressed in the terms herein adopted by us is 6968×10^{-6} , whereas that found by us at 52.87 per cent. is 7025×10^{-6} or a difference of 57×10^{-6} , a difference readily explained, having regard to the somewhat crude analytical methods adopted by Kolb.

Further, both the ascending and descending branches consist of a series of straight lines with the exception of the portion between 95 to 100 per cent., which is markedly curved.

The Ascending Branch consists in all three cases examined of four segments, namely, (i) a steep portion from 0 to 19 per cent. with a slight variation at about 4 per cent., (ii) a less steep portion from 19 to 33 per cent., (iii) a flatter portion from 33 to 42 per cent. and an almost horizontal portion at 42 to 53 per cent.; in all cases the percentages are given in round figures.

The equations for the several straight lines* have been put into the form $C_1 = C \pm a (\Delta p)$, wherein C_1 is the contraction to be calculated, C the contraction at the selected origin of the co-ordinates, a a constant and Δp the difference of percentage. The value of C was determined graphically, being the point of intersection of the two several branches.

The values for a are summarised for the purpose of convenience in the following table:—

	Percentage.	At 4° .	At 14.2° .	At 24.2° .
I.....	$\left\{ \begin{array}{l} 0-4 \\ 4-20 \end{array} \right.$	$\begin{array}{l} +210 \\ +200 \end{array}$	$\begin{array}{l} +197 \\ +190 \end{array}$	$\begin{array}{l} +190 \\ +182 \end{array}$
II.....	20—33	+155	+152	+145
III.....	33—42	+115	+100	+100
IV.....	42—54	+ 30	+ 30	+ 45

* For drawing mean straight lines for a number of observations a flat glass ruler with bevelled edge has been found convenient and more expeditious than the common forms of opaque rulers.

It will be observed that the differences in the values of the constant are considerable, with the exception of the slight variation at 4 per cent., upon which it is not desired to lay stress; further, the values of the constant are fairly simple integers.

The *Descending Branch* consists of five segments, (i) a rather flat portion from 54 to 63 per cent., (ii) a steep portion from 63 to 70 per cent., (iii) a more steep portion from 70 to 78 per cent., (iv) an abrupt descent from 78 to 95 per cent., and (v) a curved portion from 95 to 100 per cent.

The values for the constant α are summarised in the following table:—

	Percentage.	At 4°.	At 14°·2.	At 24°·2.
V	54—63	— 88	— 70	— 73
VI	63—70	—100	—100	— 85
VII	70—77	—157	—150	—122
VIII	77—94	—205	—208	—195

It will be observed that the difference in value for the portions V and VI is not considerable.

As to Segment IX, or curved portion, it was thought at the earlier stage of the inquiry that the apparently irregular results obtained with acids of percentage strengths from 95 to 100 per cent. might arise from experimental errors, the possibility of which is greatly increased in the case of solutions difficult to manipulate and of high coefficient of expansion. With a view of deciding the matter, fresh samples of acids were prepared *de novo* after an interval of about fifteen months; concordant results were obtained in the case of two samples of different past history and of approximately the same concentration, namely, 92·34 and 92·66 per cent. respectively, which pointed to an absence of any substantial error in the earlier work. Further determinations made with samples of concentration intermediate between those formerly made gave intermediate values.

The results are illustrated by certain curves, for a description of which the reader is referred to pp. 100–105.

Effect of Temperature.—Though the mean straight lines representing the observations are not parallel with one another for the three temperatures, yet within the limits 4°–24°·2 there does not appear to be any great variation in the percentage composition corresponding to the critical points as conditioned by temperature. It is very probable that at higher temperatures such variations would occur,

and some, if not all, of the critical points would be eliminated; unfortunately for the demonstration of such a probability, nitric acid of 100 per cent. concentration or thereabouts cannot be heated under ordinary conditions much above 25° C. without increasing considerably the risk of decomposition, which would render such an acid useless as a standard.

VIII.—*Discussion.*

If the minor variation at 4 per cent. be dismissed as inconsequent, it will be observed that the principal critical points are, in all three cases of temperature examined, in the vicinity of 20, 33, 42, 54, 63, 70, and 78 per cent.; the variation at 94 per cent. will be discussed separately. The first of the series has already been observed by Pickering in plotting out Berthelot's results for the heat of dissolution, and those of Kolb for the contractions (*loc. cit.*), and would correspond approximately to a hydrate, $\text{HNO}_3 \cdot 14\text{H}_2\text{O}$ (20·31 per cent.), the second to the composition of a hydrate, $\text{HNO}_3 \cdot 7\text{H}_2\text{O}$ (33·33 per cent.), the third to that of the hydrate $\text{HNO}_3 \cdot 4\text{H}_2\text{O}$ (41·18 per cent.), the fourth to that of the hydrate $\text{HNO}_3 \cdot 3\text{H}_2\text{O}$ (53·93 per cent.), the fifth to that of the hydrate $\text{HNO}_3 \cdot 2\text{H}_2\text{O}$ (63·63 per cent.), the sixth to that of the hydrate $2\text{HNO}_3 \cdot 3\text{H}_2\text{O}$ (70 per cent.), and the seventh to that of the hydrate $\text{HNO}_3 \cdot \text{H}_2\text{O}$ (77·77 per cent.). Of these the second, third, fourth, fifth, and seventh have frequently been alluded to in recent literature (*vide* summary in previous paper, *loc. cit.*), so that it will be unnecessary to recapitulate the evidence. As regards the sixth point at 70 per cent., there appears to have been a considerable diversity of opinion. Since the acid obtained by the distillation, whether of more concentrated or more dilute acid, has an approximate composition of 68 per cent., older writers regarded the acid as consisting of the hydrate $2\text{HNO}_3 \cdot 3\text{H}_2\text{O}$ deprived of a small proportion of acid by volatilisation. But as Roscoe* showed that the composition of the distillate varied with the conditions of pressure, the former view was abandoned, and it was held that the constancy of the distillate was merely dependent upon the particular conditions of atmospheric pressure under which the greater proportion of mankind lives. More recent writers (Mendeleeff, Erdmann) have been impressed with the theoretical significance of the hydrate $\text{HNO}_3 \cdot 2\text{H}_2\text{O}$ as the ultimate possible combination of nitrogen with hydroxyl, namely, $\text{N}(\text{OH})_5$, and therefore regard the acid of constant composition mentioned above as consisting of the hydrate deprived of a small proportion of water by volatilisation. These writers adduce as evidence that if dry air at 15° C. be passed, whether through more concentrated or more dilute acid, the final product in either case contains 64 per

* 'Chem. Soc. Journ.,' 1861 (i), p. 147.

cent. acid, or approximately of the composition $\text{HNO}_3 \cdot 2\text{H}_2\text{O}$; further, Pickering (*loc. cit.*) has obtained crystals of this composition by freezing. As regards the contractions, the change at 70 per cent. appears to be more marked than that at 63 per cent., and it might be a matter of judgment if the results from 54 to 70 per cent. might not equally well be represented as upon one curve and not upon two straight lines. The latter view has been adopted to bring the results obtained within these limits into uniformity with the rest.

As regards the abnormality at 95 to 100 per cent., it is probable this is the result of a wholly different cause; a similar phenomenon was observed in the electric conductivity, and the suggestion was put forward of an initial decomposition into water and nitric anhydride, but since the latter, when passed into nitric acid of about 100 per cent. concentration, forms a substance of composition $2\text{HNO}_3 \cdot \text{N}_2\text{O}_5$ or $2\text{N}_2\text{O}_5 \cdot \text{H}_2\text{O}$ (a colourless liquid of specific gravity 1.642 at 18°, crystallisable at -5°), both the contraction and the electric conductivity numbers might be expressed in terms of this substance, and not in terms of nitric acid 2HNO_3 or $\text{N}_2\text{O}_5 \cdot \text{H}_2\text{O}$. At present the physical constants of the crystallisable compound have not been sufficiently examined.

IX.—The Degree of Discontinuity.

Though in the diagrams of curves the mean straight lines are produced until they intersect, yet the object thereof is to determine each separate origin of co-ordinates; it is not intended to represent an irregularity occurring suddenly or an abrupt change of events. The results in themselves show one or two possibilities at each critical point, namely, firstly, a very gradual transition from one phase to another, so that the observation points within about a 2 per cent. limit could equally well be represented on either of the lines; secondly, a transition stage, in which the observation points lie upon neither of the straight lines, but one generally lower than both. Examples of each of these two possibilities will be discussed *seriatim*.

Firstly, a gradual transition. An example of this can be illustrated from the results obtained at 24.2 for acids of percentage values from 30.17 to 33.87, and in the following table (p. 98) the observed results for the contractions are compared with those calculated both from the 21 per cent. and the 33.3 per cent. origins of co-ordinates.

It is evident that the result for the 30.17 per cent. acid belongs to the straight line drawn from the 4 per cent. origin, and the result for 33.87 per cent. acid, on the other hand, belongs to the straight line drawn from the 33.3 per cent. acid, while those intermediate could within the degree of experimental error be equally represented as belonging to either line.

Percent- age.	Contraction observed $C \times 10^{-5}$.	Contraction calculated from the 21 per cent. origin.	Difference.	Contraction calculated from the 33·3 per cent. origin.	Difference
30·17	5171	5144	+ 26	5227	- 56
31·63	5350	5362	- 12	5373	- 23
32·05	5441	5427	+ 14	5415	+ 26
32·82	5510	5539	- 29	5492	+ 18
33·14	5540	5585	- 45	5524	+ 16
33·87	5571	5691	- 120	5597	- 26

A second example taken from the descending portion, namely, 1 acids of percentage values from 60·60 to 65·80 per cent., illustrate the same phenomenon.

Percent- age.	Contraction observed.	Contraction calculated from the 54 per cent. origin.	Difference.	Contraction calculated from the 63·6 per cent. origin.	Difference
60·60	6592	6584	+ 8	6650	- 58
61·53	6528	6517	+ 9	6566	- 32
62·84	6399	6427	- 28	6373	+ 26
63·83	6327	6399	- 72	6327	nil
65·80	6104	6218	- 114	6130	- 26

Here also the result for the 60·60 per cent. acid belongs to 1 straight line drawn from the 53·8 origin, and conversely those : the 63·83 and the 65·8 per cent. acids belong to the straight li drawn from the 63·6 per cent. origin, while the remaining two c equally be represented as belonging to either line.

Secondly, a transition stage, in which the observation points upon neither of the straight lines. This is best exemplified by 1 results obtained at all three temperatures for the alteration at ab 20—21 per cent. concentration, which was more particularly inve gated in consequence of the divergence between the observed a calculated results for the acids of about 19·5 per cent. concentration.

In the three succeeding tables the observed contractions are cc pared with those calculated either forwards or backwards from 1 two respective origins of co-ordinates.

Results at 4°.

Percent-age.	Contraction observed.	Contraction calculated from the 4 per cent. origin.	Difference.	Contraction calculated from the 21 per cent. origin.	Difference.
18·44	3702	3738	-36	3758	-56
19·37	3881	3924	-43	3912	-31
20·68	4022	4168	-164	4105	-73
21·43	4181	4346	-165	4222	-41
21·95	4291	4440	-149	4302	-11

Results at 14·2.

19·37	3685	3706	-21	3712	-27
19·72	3722	3767	-43	3756	-34
20·24	3803	3876	-73	3834	-31
20·68	3849	3959	-110	3900	-51
21·38	3952	4096	-144	4008	-56
21·98	4087	4206	-119	4099	-12

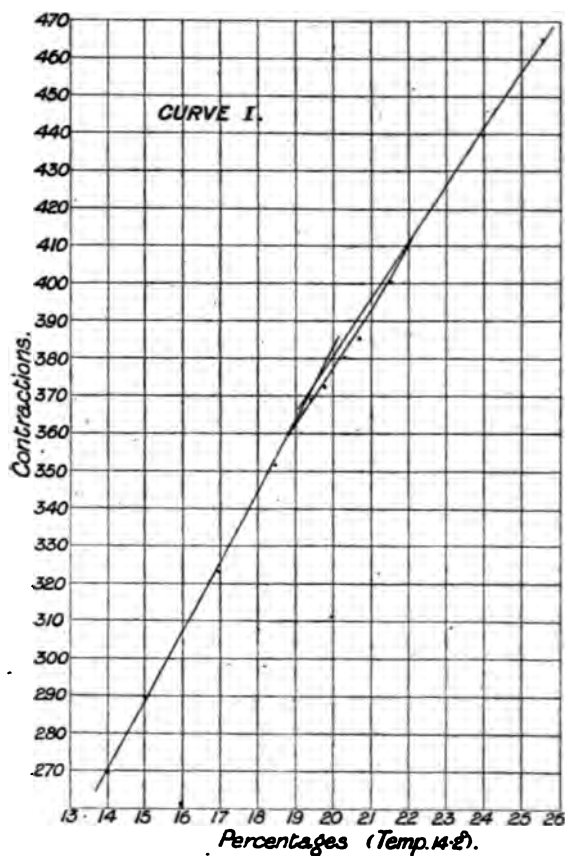
Results at 24·2.

18·44	3360	3389	-29	3454	-94
19·37	3486	3541	-51	3589	-103
20·24	3688	3717	-29	3715	-27
20·68	3724	3796	-74	3779	-55
21·43	3822	3932	-110	3887	-65
21·98	3959	4032	-63	3967	-8
22·37	4016	4103	-89	4024	-8

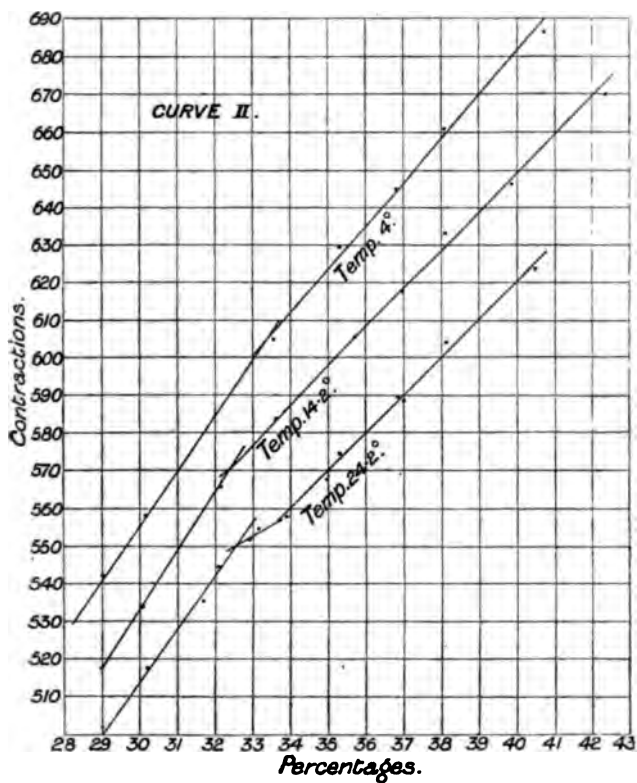
It will be evident that in all cases the results up to 19·37 lie upon the straight line drawn from the 4 per cent. origin, and on the other hand those 21·98 upwards lie upon the straight line from the 21 per cent. origin, while the intermediate values lie below both lines. As it would appear from a consideration of the examples given above that the observations in the vicinity of critical points may be represented in certain cases as on either, and in others as on neither of the lines meeting at those points, then such points may be regarded as in a certain sense ideal, and the phenomena as continuous. Some such possibility might tend to overcome the mental difficulty experienced by certain writers as regards the manifestation of sudden irregularities, and throughout the whole course of events from commencement to conclusion one is dealing with combined water and free water, with combined acid and free acid, and in the more dilute solutions with integral or undecomposed molecules as also with their respective ions.

X.—Description of Curves.

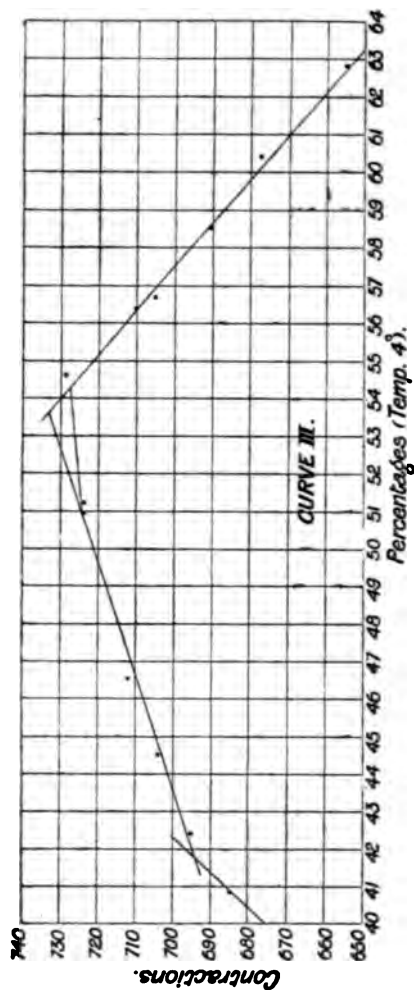
Curve I.—Results at 14.2 from 14 to 26 per cent., with intersection of lines at 19.6 per cent., the values from 19.6 to 21.5 per cent. lower or intermediate line.

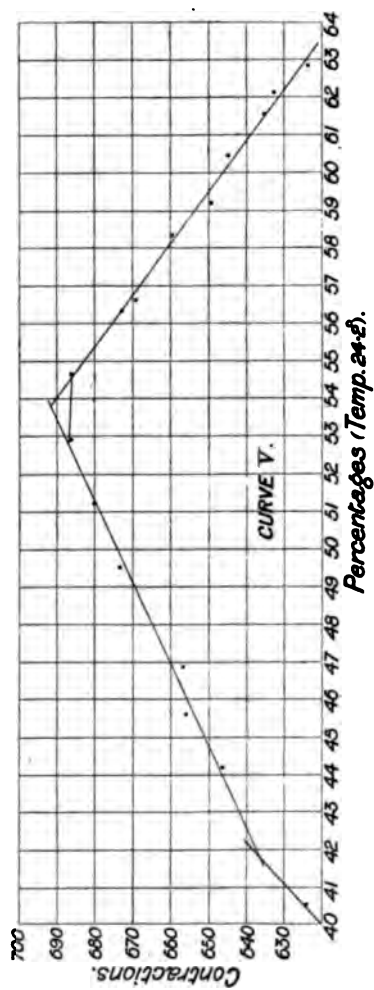
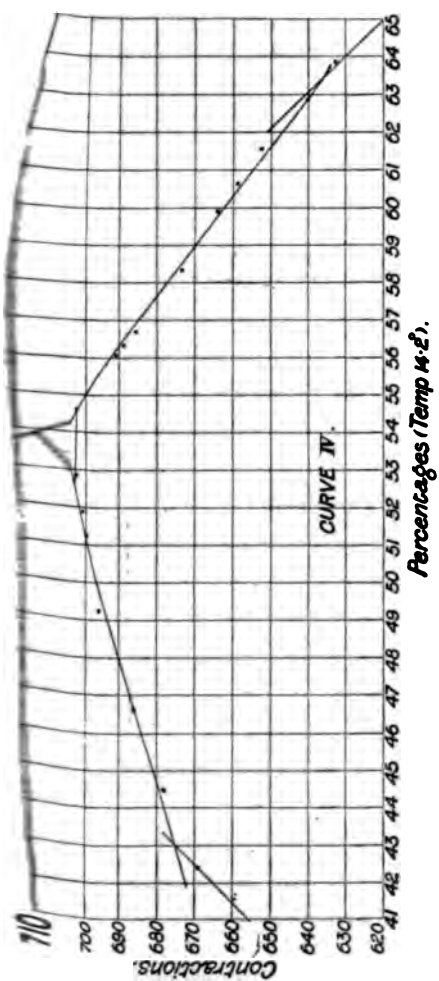


Curve II.—Results at the three temperatures from 28 to 42 per cent. at intersection of lines at about 33 per cent. ; in the lowest curve results slightly above and below 33 per cent. are represented on a cross line.

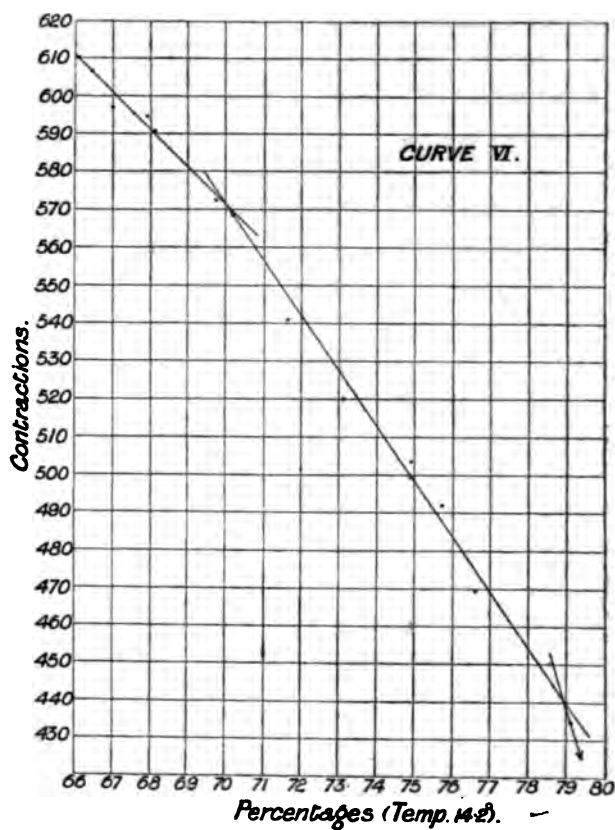


Curves III to V.—Results at the three temperatures from 40 per cent. with the point of maximum contraction; the ascending descending branches are protracted to their point of intersection the nearest results on either side joined by a straight line.

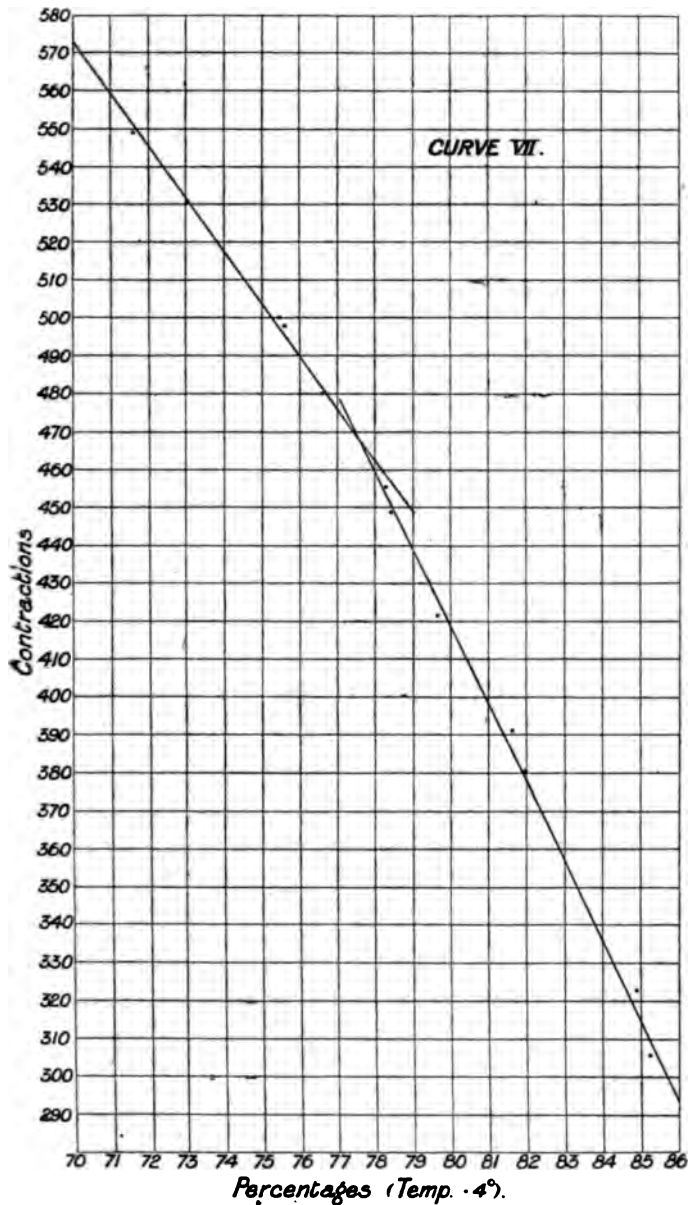




Curve VI.—Results at 14.2 from 66 to 80 per cent., with intersec at 70 per cent., and another (direction only shown) at 79 per cent



Curve VII.—Results at 4 per cent. from 70 to 86 per cent., with intersection at 77·8 per cent.



All these curves were drawn originally on the same scale, namely, 1 mm. for 0·1 per cent. and 0·0001 c.c. contraction ; only a selection is given as illustrations.

SECTION II.—*The Refractive Indices of Nitric Acid.**Introductory.*

The earliest observation on record upon any one sample of nitric acid appears to be that of Baden-Powell,* namely, μ_D^{25} 1.4026, but the percentage concentration is not given. Twenty-seven years later van der Willigen† published values of μ for various spectrum lines from A to H determined at the same temperature from which the absolute index (Cauchy's method) is deduced from the formula

$$n = 1.385967 + 0.527631 \lambda^{-2} - 0.18099 (10^6) \lambda^{-4}.$$

The values of μ for the same line at different temperatures are also given, from which $\Delta\mu/\Delta t = 0.000424/1$ is deduced.

Owing to experimental difficulties, especially in the measurement of μ , only one particular sample was fully examined by v. d. Willigen namely, of concentration 58.89 per cent.

His work will, however, be alluded to frequently in the sequel for the following reasons: (i) his instrument was by the same maker and of the same construction; (ii) his methods present certain points of similarity; and (iii) his more detailed observations on mixtures of sulphuric acid and water showed that Biot's and Arago's formula—

$$(100 - p)(\mu^2 - 1)/d + p(\mu'^2 - 1)/d' = 100(\mu''^2 - 1)/d''$$

—was inadmissible in such a case, not of admixture, but of presumed chemical combination, but that variations of μ for the same liquid at different temperatures can be expressed by a differential:

$$\Delta\mu = (\mu^2 - 1)\Delta d/2\mu d.$$

Gladstone,‡ in the course of his prolonged investigations on Refraction Equivalents $M(\mu - 1)d$, has from time to time given values for different spectrum lines; these results in the particular cases of R_α , R_β and R_γ , for samples of percentage values varying from 6 to 98.7 per cent. have been collated together by the above-mentioned observer with Hibbert,§ from which the average rate of change per difference of percentage, namely, $R_2 - R_1/p_1 - p_2$, is calculated. Ten samples from two specimens of percentage values varying from 6 to 98.7 per cent. have been examined, and the experimental results illustrated by diagrams of curves.

The general conclusion arrived at is "that the refraction equivalent

* 'The Undulatory Theory,' p. 112 (London, 1841).

† 'Archives Musée Teyler' (1), 79 (Haarlem, 1868), and (2), 238 (1869).

‡ 'Roy. Soc. Proc.' vol. 16, p. 439; 'Phil. Trans.' 1870; 'Journ. Chem. Soc. (Trans.)', 1891, p. 589.

§ 'Journ. Chem. Soc.' (Trans.), 1895, p. 831.

of the anhydrous acid is increased by dilution until it has combined with about four times its weight of water, beyond which there is an apparent decrease." This alteration would correspond to an alteration at or about a percentage value of 20 per cent., and will be alluded to more especially in the sequel.

In another passage the authors express themselves as follows: "We have little doubt of the existence of hydrates in solution, but our experiments do not give any clear evidence of them," and "though we have looked for coincidences between probable hydrates in solution and changes of curvature in our diagrams, we have not found any that we could distinctly recognise."

As to these observations the authors would venture to express the opinion that the percentage values, for which observations were made, were possibly in this particular case too few in number to enable any conclusions to be drawn definitely therefrom.

Le Blanc* gives determinations of four samples of percentages varying from 14.09 to 69.18 per cent., and arrives at the same general result as Gladstone, that the values of the refraction equivalents increase as the percentage decreases, and as the result is in accord with that deduced from an examination of other substances, it is cited as evidence in favour of the dissociation hypothesis of solution.

Lastly Pulfrich,† from an examination of certain solutions, has traced out a relation between the alteration of refractive indices and contractions on admixture, as expressed by the equation

$$N - N_v/N = a(D - D_v)/D,$$

in which N and N_v are the values of $n - 1$, D and D_v those of the densities (i) for the mixture as observed, and (ii) for the mixture if no alteration in volume had occurred, while a is a constant.

It will thus be evident that former observers have viewed their results from the three different standpoints of (i) the coexistence of unaltered and uncombined molecules of water and acid respectively; (ii) the dissociation to a greater or less degree of the acid molecules into their constituent ions; and (iii) an aggregation of some kind or another of water and acid molecules, if contraction in volume is taken as a criterion of chemical combination.

It has already been shown by one of us‡ that the determination of refractive indices is a method as delicate as that of densities for differentiating samples of water of different degrees of salinity.

* 'Zeits. Physikal. Chem.,' vol. 4 (1889), p. 533.

† 'Zeits. Physikal. Chem.,' vol. 4 (1889), p. 561.

‡ 'Roy. Soc. Proc. Edin.,' vol. 23 (1900), p. 36.

II.—*The Spectrometer.*

The instrument, for the loan of which we are indebted to the Government Grant Committee of the Royal Society, was obtained from Becker (Meyerstein's successor) originally for the purpose of investigating the method of determining the specific refractivities of solids from their solutions.* Its graduated circle is 27 cm. diameter, and reads directly to one-tenth degree; it is furnished with two reading microscopes, supplied with micrometers, three revolutions of which correspond to one scale division; as the micrometer screw heads are divided into sixty parts, an observer is enabled to read directly to two seconds.†

III.—*The Prism and its Fittings.*

As it was thought probable that the more concentrated acids would corrode a glass prism, and the saline materials dissolved out would alter the refractive indices of the samples, a prism entirely of quartz was constructed under our direction by Mr. Hilger. The dimensions of its plates, cut with their planes at right angles to their optic axes, were 75 mm. $\times$ 68 mm. $\times$ 6 mm. diameter, of the aperture 20 mm., and the angle of the prism about $60^{\circ} 6'$, an accurate measurement of which is given in the sequel. At the upper end a hole was drilled so that a thermometer graduated to 0.1° C. could be introduced, when required, into the prism contents.

Solid paraffin was selected as likely to be the best material for cementing the plates on to the main block; the paraffin was purified by boiling with concentrated nitric acid, and then frequently with water, until all the acid had been washed out, dried at 90° in a water oven, and thus a material of constant melting point 51° obtained.

The process of cementing was conducted as follows: The central block and its plates were cleaned and the former placed upon a wood slab so that the refracting angle was uppermost; the plates were placed in their proper position and the whole introduced into a water oven, the temperature of which was gradually raised to 80° ; the prism with the slab of wood was then lifted out and the thinnest possible film of paraffin run in by capillary attraction by means of a feather until the film had penetrated almost to the aperture; the whole was then replaced in the oven and allowed to cool down slowly. By this process any strain due to an unequal rate of cooling was avoided.

* 'Brit. Assoc. Report,' 1881, p. 155.

† A full description of a similar instrument is given by van der Willigen (*vide supra*).

IV.—Parallelism of Quartz Plates.

This was ascertained (a) by an optical method, (b) by a spherometer reading to 0.001 mm. In the first method the two plates A and B were placed successively in a vertical position on the prism table of the spectrometer, which was turned until one edge of the image of the reflected slit of the collimator coincided with the intersection of the cross threads; micrometer readings were then taken. The divided circle was then rotated until the image of the same portion of the slit was seen reflected from the other face of the plate, and a second set of readings taken. The whole series of operations was repeated several times and with different parts of the circle. By this method the faces of one plate A were found to deviate from true parallelism by 12" of arc, while those of the other plate B were optically parallel.

In the second method the thickness of each plate was determined at four points, each about 2.5 cm. from the four angles along the diagonals; the results were as under :

Plate A.	Plate B.
6.056 mm.	6.044 mm.
.049 "	.043 "
.042 "	.045 "
.049 "	.043 "
Mean... 6.049 "	Mean... 6.044 "

Both of these methods show that no sensible error could arise from imperfect parallelism.

V.—Angle of Prism.

The refractive indices of twenty-four samples were determined with a refractive angle of prism of $60^{\circ} 8' 29''$ (mean value of numerous concordant measurements), the remaining sixteen samples with a refractive angle of $60^{\circ} 6' 7''$ (mean value also of concordant measurements); in every case the angle was measured by the usual method of keeping the telescope of the spectrometer in a fixed position and rotating the prism.

It is possible that the former and higher value arises from some fragment of dust or extraneous matter between one of the quartz blocks and body of prism, and the supposition is apparently borne out by the fact that though the prism has been subsequently re-built on frequent occasions, yet the latter and lower value has invariably been obtained.

VI.—*Effect of Temperature on Refracting Angle.*

In the course of the work numerous determinations of the refracting angle were made at different temperatures; of these the following pair may be cited, each value being the mean of several determinations:

Temperature.	Refracting angle.
11·6	60° 6' 4"
19·0	60° 6' 2"

This difference is quite insignificant. It is possible that differences in the refracting angle at different temperatures, noticed by previous observers, may be due not to any alteration in the prism by itself, but to the expansion of the wax or other cementing material, as to the thickness of which and the method of spreading it details are not given.

VII.—*Temperature.*

In the more recent experimental work upon refractive indices the prism has generally been enclosed in some form of jacketing arrangement through which a stream of water at about the required temperature was allowed to flow. Some of the arrangements suggested increase the difficulty of manipulating the prism in any direction, while for every case it appears probable that, if observations are required at a temperature considerably above that of an ordinary temperature, an optical strain might result from convection currents from the hot jacket, and thus observations at a higher might not be comparable directly with those at a lower temperature.

Some observers have remarked upon the number of constants required for an empirical equation—

$$\mu' = \mu'(1 + at + bt^2 + ct^3 \dots)$$

—to fit the observations; possibly the explanation may be found in such a strain as described above.

As in this particular investigation the substance under examination may begin to decompose at a temperature of about 30°, the plan was adopted of altering the whole temperature of the room so that not only the prism, but also the lenses, telescope fittings, micrometer scales, &c., should be affected *pro rata* as regards their individual coefficients of expansion.*

It was judged that, when the thermometer in the prism contents and a second of the same order of delicacy placed on the rotating table registered the same temperature for a time longer than that

* This method of working was adopted by van der Willigen in determinations of the variation in refractive index of glass prisms with temperature.

required, it would be safe to make all the necessary observations and readings.

Our experience has been that if by any chance the prism-contents were altering in temperature the edges of the D-line appeared somewhat blurred, but if the temperature was constant the line was perfectly sharp and well defined. Though this method of working is accompanied by some personal discomfort, yet it is believed to be susceptible of greater accuracy than when only the temperature of the prism is varied.

VIII.—*Refractive Index of Water.*

As a preliminary experiment determinations were made of μ_D for water, purified by the methods adopted by us; the values found were $\mu_D^{14.2} = 1.33376$ and $\mu_D^{18} = 1.33328$; the latter does not differ widely from the mean value $\mu_D^{20} = 1.33334$ obtained by former observers, whose results have been recently collated by Rudolphi.*

IX.—*Method of Observation.*

The method of conducting a determination was as follows :—

- (i) The prism was washed out four times with portions of the prepared sample of acid, then finally charged, allowed to stand for a short time and the thermometer inserted;
- (ii) Spectrometer telescope adjusted for viewing the collimator slit directly, the point of intersection of the cross lines being made to coincide with the left-hand edge of the image of the slit, and a reading taken;
- (iii) Charged prism placed upon the table and the position of minimum deviation for the D-line found, the temperature being noted;
- (iv) Prism removed and (ii) repeated;
- (v) Repetition of (iii) after a short interval. After several hours a second set of experiments was made.

The following are the details of one particular set of determinations :—

Date, 12.4.1900. Concentration of acid = 71.57 per cent.

First set, temp. 15.3 (corr.). Hour, 2.50 P.M.

	Micrometer No. I.	Micrometer No. II.
Direct readings	180° 0' 6"	360° 0' 8"
Deviation	150 36 4	330 35 56
Difference = Min. Dev.	29 24 2	29 24 12
Mean...	29° 24' 7".	

* 'Zeits. Physikal. Chem.,' vol. 37 (1901), p. 483.

Second set, temp. 13° (corr.). Hour, 7.45 P.M.

	Micrometer No. I.	Micrometer No. I X.
Direct readings	180° 0' 6"	360° 0' 8"
Deviation	180 31 12	330 31 0
Difference	29 28 54	29 29 8

Mean value = 29° 29' 1".

In the interval which elapsed between the two sets of observations the thermometer was removed and its aperture covered by a quartz plate. If the two mean values given above are corrected to the same standard temperature, 14° 2', the value of the former becomes 29° 26' 27", the latter 29° 26' 28", or a difference of 1".

X.—Sources of Error and their Magnitude.

I. *Effect of Prism on the Acid.*—In order to ascertain if any chemical action occurred between the quartz and the acid, the following experiments may be cited as a selection of those conducted. Three samples of 3.86, 70.04, and 91.29 per cent. concentration respectively were chosen, and the value of their refractive indices determined (i) as soon as possible after introduction and (ii) after the expiry of a known period of time. The values are given in the following table:—

Percent- age strength.	Time.	First value of $\mu_D^{14.2}$.	Final value $\mu_D^{14.2}$.	Difference.
3.86	4 hours	1.338442	1.338424	-0.000018
70.04	26 "	1.406094	1.406087	-0.000007
91.29	9 "	1.400990	1.400997	+0.000007

The differences given in the fifth column are within the limit of experimental error, and therefore the amount of substance which might have been dissolved out would not materially alter the value of μ .

II. *Effect of Paraffin on the Acid.*—Into a sample of 29.4 per cent. concentration ($\mu_D^{14.2} = 1.374181$) a quantity of the above-mentioned purified paraffin in the form of fine shavings was introduced; the thermometer hole was closed with a well-polished plate of quartz and the whole allowed to stand at ordinary temperature. At the end of 18 hours determinations gave the value $\mu_D^{14.2} = 1.374252$ and at the end of 42 hours a value $\mu_D^{14.2} = 1.374258$, or variations of +0.000056 and +0.000077 respectively.

It will be evident that in this experiment, purposely conducted in degassed form, the difference produced is very small, and hence it may safely be concluded that no appreciable effect can be caused by a very thin film of paraffin in experiments lasted for 3 hours on an average.

Glass of Thermometer introduced into the Acid.—For the greater part of time during which the experiments were conducted the thermometer hole was closed with a quartz plate; as the thermometer was made of lead glass, was introduced for only a short time it may reasonably be assumed that the quantity of glass dissolved out under these conditions would be insignificant.

Personal and Experimental Error due to Micrometer Screw.—The readings of adjustments were probably accurate to $\pm 2''$ or a possible error of $4''$, a magnitude which would change the value of the weakest acid by ± 0.000019 and for the strongest of 0.008; the values for μ are therefore correct in most cases to the fifth place with a variation in the sixth place.

XI.—Temperature Coefficients.

In order to determine the above values, the sample alluded to above is given as an illustration and the following results cited:

Temperature (corr.).	Value of μ_D .
11.6	1.407157
18.1	1.409349
6.5...	Differences ... 0.002808

which a decrease in μ of 0.000432 is obtained for increase of temperature; two other determinations gave $\mu_D^{15.5} = 1.405558$ and $\mu_D^{18} = 1.405567$. Accepting this temperature coefficient there are four values for the reduction of μ_D to the standard temperature, 14.2, and the degree of concordance is shown below:

Temperature.	μ_D reduced to 14.2.
11.6	1.406034
13.0	1.406049
15.3	1.406033
18.1	1.406034
Mean ...	1.406037

In the same manner the temperature coefficients expressed in terms of μ_D have been determined for all the other samples of acids and the values reduced to standard temperature as shown above. The following table gives the coefficients:

Percentage concentration.	Temperature coefficients 10^{-6} .	Percentage concentration.	Temperature coefficient 10^{-6} .	Percentage concentration.	Temperature coefficients 10^{-6} .
2.19	95	41.45	293	75.23	447
3.85	94	44.69	326	78.07	447
6.76	108	46.90	340	81.12	484
9.29	122	49.12	352	84.11	441
11.88	128	51.00	372	85.86	442
16.11	156	54.63	386	87.26	455
18.27	177	57.00	388	89.88	441
21.66	190	60.33	406	91.24	465
25.07	217	63.07	420	92.82	468
29.40	231	66.60	419	95.66	483
32.50	267	67.92	432	98.67	484
34.26	280	70.04	418	99.87	480
36.68	279	71.51	432		
39.11	292	74.63	433		

It will be observed that these temperature coefficients increase gradually but by no means uniformly with increase of percentage; in both cases only very small differences are measured, which accounts for the slight irregularities. There is probably some function connecting these two factors, but from certain trials made it does not appear to be of a simple character.

The value for the 60.33 per cent. acid, namely, 0.000406 is not widely different from that 0.000424 given by van der Willigen for his sample of 58.89 per cent. concentration.

XII.—Results.

- The results for μ_D reduced to a uniform temperature are set forth in the following table:—

Percentage concentration.	$\mu_D^{14.2}$.	Percentage concentration.	$\mu_D^{14.2}$.	Percentage concentration.	$\mu_D^{14.2}$.
2.19	1.336208	41.45	1.388788	75.23	1.405599
3.85	338434	44.69	392305	78.07	405091
6.76	342298	46.90	394496	81.12	404504
9.29	345824	49.12	396699	84.11	403427
11.88	349371	51.00	398435	85.86	402924
16.11	355107	54.63	401017	87.26	402412
18.27	358541	57.00	402440	89.88	401419
21.66	363091	60.33	404080	91.24	401017
25.07	365152	63.07	405157	92.82	400249
29.40	374225	66.60	405860	95.66	398148
32.50	377798	67.92	405985	98.67	396980
34.26	380354	70.04	406094	99.87	397167
36.68	383030	71.51	406048		
39.11	386262	74.63	405696		

It will be observed that the values for μ_D increase slowly with increase of percentage up to 70 per cent., the maximum value; from this point the values decrease up to 98.67 per cent., when there is a slight but apparent reversal. The value for the anhydrous acid is approximately equal to that of a mixture in equal proportional weights of acid and water, a somewhat curious coincidence. This maximum value appears to have escaped the notice of previous workers on the refractive indices of nitric acid.

In the following table the values are given for $(\mu_D - 1)/d$, Gladstone and Dale's formula, and for $\mu_D^2 - 1/(\mu_D^2 + 2)d$, Lorentz's formula; the ratio of the two values is approximately 1.63 to 1, the densities in both cases have been interpolated from our results, the fifth decimal place being dismissed as inconsequent, and the assumption being made that at least for small differences in percentages the values may be represented as on straight lines.

In the case of both factors the values decrease with increase of percentage, the variations for that of Gladstone and Dale amounting to 21.4 per cent., and for that of Lorentz to 23 per cent. of the whole values.

Gladstone (*loc. cit.*, *supra*) has shown previously that the molecular refractive energies of nitric acid, as determined from aqueous solution, and after taking into account the water present, are not constant, though the variations deduced by this method of representation are not of the same order of magnitude.

The observed results for one of the above factors (Lorentz) are compared with results calculated from formula of straight lines $M_1 = M - a(\Delta p)$, wherein M_1 is the deduced value, and M the values for $\mu_D^2 - 1/(\mu_D^2 + 2)d$ for the several origin of co-ordinates, a is a constant and Δp the percentage differential. Neglecting the first two values, which stand by themselves, the value for M from 6 to 20 per cent. is taken at 0.20360, and for the remaining portion at 0.19750.

Percentage.	$\mu - 1/d \cdot 10^{-5}$. Observed.	$\mu^2 - 1/(\mu^2 + 2)d \cdot 10^{-5}$. Observed.	$M_1 = M - a(\Delta p)$. Calculated.	Difference.
2.19	33228	20507	—	—
3.85	33169	20453	—	—
—	—	—	$M = 20360,$ $a = -4.1$	—
6.79	33012	20339	20329	+ 10
9.29	33093	20222	20225	- 3
11.08	32597	20118	20118	- 7
16.11	32472	199935	19946	- 11
18.27	32356	19853	19857	- 4

Percentage.	$\mu - 1/d10^{-5}$ Observed.	$\mu^2 - 1/(\mu^2 + 2)d10^{-5}$ Observed.	$M_1 = M - a(\Delta p)$ Calculated.	Difference.
—	—	—	$M = 19750,$ $a = -4.2$	—
21.66	32166	19702	19680	+ 22
25.07	31956	19550	19537	+ 13
29.40	31713	19362	19355	+ 7
32.50	31470	19185	19175	+ 10
—	—	—	$a = -4.6$	—
34.36	31328	19098	19100	- 2
36.68	31141	18982	18983	- 1
39.11	30985	18853	18871	- 18
—	—	—	$a = -4.8$	—
41.45	30792	18722	18721	+ 1
44.69	30574	18571	18566	+ 5
46.90	30325	18450	18449	+ 1
49.12	30241	18351	18352	- 1
51.0	30096	18240	18262	- 22
—	—	—	$a = -4.85$	—
54.63	29778	18041	18071	- 30
57.0	29650	17956	17956	Nil
60.33	29385	17785	17794	- 9
63.07	29168	17651	17660	- 9
—	—	—	$a = -5.0$	—
66.60	28909	17430	17420	+ 10
67.92	28738	17373	17369	+ 4
—	—	—	$a = -4.95$	—
70.04	28576	17297	17280	+ 7
71.37	28434	17207	17195	+ 12
74.63	28130	17021	17046	- 25
75.33	28063	16980	17016	+ 26
78.07	27833	16552	16876	- 24
81.12	27693	16761	16725	+ 36
84.11	27360	16565	16576	- 15
85.80	27256	16505	16493	+ 12
87.20	27203	16369	16424	- 55
89.88	26899	16296	16291	+ 5
91.29	26792	16232	16221	+ 11
92.82	26680	16159	16146	+ 13
96.66	26481	16057	16005	+ 52
98.67	26182	15895	15877	- 8
99.87	26090	15824	15787	+ 37

It will be observed that the values for a vary at the following points: 20 per cent., previously noticed by Gladstone, 33 per cent., 41 per cent., between 51 and 54 per cent., between 63 and 66 per cent.,

ween 68 and 70 per cent. ; this last difference taken by itself, with- consideration of the alteration of μ , is not very marked. The atest differences between the observed and calculated results unt in two cases only to ± 33 parts per 10,000, but in the large ority of cases to ± 5 parts per 10,000, or even less, which is within limits of experimental error.

he observed points of alteration are in accordance with the points iced in Section I, though the last point, viz., at 78 per cent., is not e apparent. It is, of course, to be noticed that the density enters a factor alike into the contractions and Lorentz's formula, though efractivity for light is a wholly independent property. As pre- ously shown, the maximum value for contraction is at 54 per cent., e that of the refractive index is at 70 per cent. ; it would not, efore, appear probable that Pulfrich's formula (*vide supra*), which easses the relation between μ and the contraction in terms of a antant, would be applicable in this particular case. It was found by mples selected from various data that though the formula is licable approximately for results differing by small percentage ontractions, yet such is far from the case when these differences considerable.

Refractive Indices Curve.

For obtaining the curve showing the changes in the refractive ices, the value for μ for 2.19 per cent. acid was taken as unity ; in order to keep the curve within moderate limits the differences he successive values of μ were divided by 3. (See next page.)

It will be observed that the observations from 2 per cent. to about 20 per cent. lie upon one straight line, and those from 20 per cent. to ut 33 per cent. are upon a second, while those from 40 per cent. to 78 per cent., the maximum, and beyond apparently lie upon some n of regular curve. The observations, however, are too few in umber, and the scale too small to decide as to whether this curve ld be resolved into two or more straight lines.

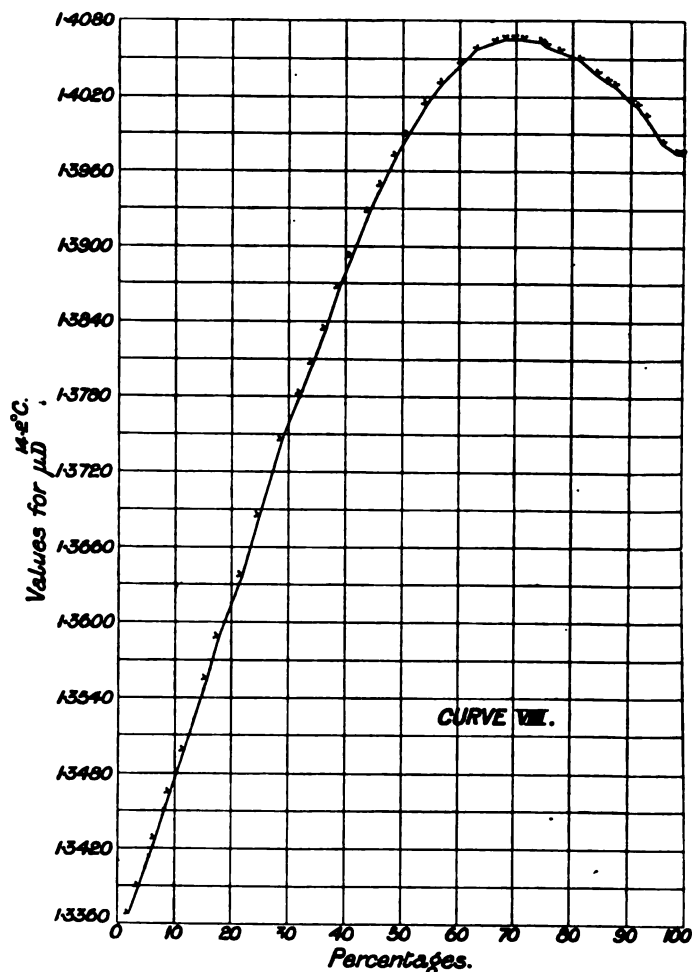
he general contour of the curve is very similar to those given by . Willigen for μ (A . . . H) in the case of sulphuric acid ; he draws ial attention to the maximum value at 83.20 per cent., and while itting that his observations are insufficient to show whether the lts from 0 per cent. to 80 per cent. are on one or several ight lines, yet alludes to the regularity of curvature in the ity of the maximum.

General Conclusions.

both the physical properties of nitric acid solutions herein iced, namely, densities or contractions and refractive indices, e various sources of error and their possible magnitude are

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discussed in full; in the former, those of analysis, unavoidable in this case, temperature, errors of filling pyknometers both with acid and water; in the latter, those of micrometer screws, dividing circle, parallelism of quartz plates, as also the several effects likely to be produced by the various substances with which the acid



solutions of necessity came into contact. The results obtained both methods are given in a series of tables and compared with those calculated from various equations for straight lines. They show that the physical properties display well-marked alterations at definite points—in a word, such properties are discontinuous. Further, such points correspond very approximately to the conc

trations required for simple molecular combinations *only* of nitric acid (HNO_3) with water. Some of these combinations have been alluded to by previous writers on this and kindred substances, while others have either not been examined or have formed the subject of a diversity of opinion.

In the case of the densities and contractions the best defined points of alteration correspond to the composition of the hydrates HNO_3 with 14, 7, 4, 3, 1.5, and 1 molecular proportions of water respectively, while in the case of the refractive indices, apart from consideration of density, the most marked points correspond to the 14, 7, and 1.5 hydrates. In the case of the contractions and electric conductivities, but to a less degree in that of the refractive indices, there is remarkable discontinuity at concentrations 95 to 100 per cent., which can possibly be explained rather by some decomposition than by some combination.

Further, the more detailed experiments on the contractions show that these points of discontinuity, though to some degree real, yet to another degree are ideal, in that an apparently abrupt change of events is resolved into either a gradual transition or a transition stage, which differs from either the preceding or successive phase. In the present state of knowledge it appears difficult to interpret the course of events between each phase, as there are the possibilities of at least two cases involving two entities, namely, (i) combined acid and water with free water, and (ii) combined acid and water with free acid.

Investigations into a possible case not readily provided by nature or art, of two liquids which *rapidly* mix but slowly combine each with each, might solve many of the problems of solution towards which the present memoir may serve as a slight contribution.

"The Anatomy and Development of the Stem in the Pteridophyta and Gymnosperms." By EDWARD C. JEFFREY, Ph.D.,
Lecturer in the University of Toronto. Communicated by
D. H. SCORR, F.R.S. Received July 15, 1901.

(Abstract.)

Comparatively little attention has been directed to the subject of the development of the stem. This research concerns itself chiefly with the development of the cauline fibro-vascular skeleton, since this appears to be most interesting from the phylogenetic and morphological standpoints. A study of numerous examples drawn from the main groups of the Pteridophyta and Gymnosperms has led to the conclusion that the polystelic type of Van Tieghem does not originate, as he states, by the repeated bifurcation of the epicotyledonary central

cylinder; but that the latter becomes at first a concentric fibro-vascular tube (Bündelrohr of De Bary), with gaps for the branches also or with gaps for both leaves and branches.

The tubular nature of the central cylinder in the polystelic type may become subsequently disguised by the overlapping of the gaps; by the appearance of medullary strands, derived in all the cases investigated by the writer from the inner wall of the stelar tube. It seems better to describe these conditions as adelosiphonic instead of polystelic, since the latter term implies a misconception.

In the Osmundaceæ the writer believes he has found evidence of derivation of the medullated monostelic and astelic types from siphonostelic condition with internal phloem by the degeneration of the latter.

Osmunda cinnamomea shows all stages between the polystelic and astelic conditions; *O. regalis* still retains occasionally a brown sclerenchymatous pith, while in *O. claytoniana* this phenomenon is quite absent. Similar examples of degeneracy are found among the Podiaceæ. Potonié further believes that the so-called medullated monostelic central cylinder of the Gymnosperms is derived by degeneracy of the internal phloem from such types as *Medullosa*. The writer considers that there is good evidence for regarding the so-called medullated monostelic type of central cylinder as derived by specialisation, accompanied by degeneracy, from the so-called polystelic type. Van Tieghem, and thus returns to the conception of the morphology of fibro-vascular strands set forth in De Bary's 'Comparative Anatomy.'

The study of the development of the fibro-vascular skeleton of higher plants seems to lead to the conclusion that is hardly less important phylogenetically than the osseous skeleton has proved to be in the case of vertebrated animals. Where the tubular central cylinder exists there are two main types, the phyllosiphonic, where foliar gaps are constantly present, and the cladosiphonic, where foliar gaps are equally constantly absent. The central cylinder of the Filices, Gymnosperms, and Angiosperms belongs to the former type, and that of the Lycopodiales and Equisetales to the latter. These distinctions appear to be of special importance, on account of the absence of constant and far-reaching criteria of taxonomy among the vascular plants. They moreover agree closely with evidence drawn from other available sources.

The writer is of opinion that there are two great primitive stocks of vascular plants, the Lycopsidea and the Pteropsida. The Lycopsidea include the Lycopodiales and Equisetales, and are palæogenetically microphyllous and cladosiphonic. The Pteropsida include the Filices and Phænogams, which are primitively megaphyllous and phyllosiphonic.

"A Memoir on Integral Functions." By E. W. BARNES, M.A.,
Fellow of Trinity College, Cambridge. Communicated by
Professor A. R. FORSYTH, Sc.D., F.R.S. Received July 25,
1901.

(Abstract.)

The memoir deals with the asymptotic expansion of functions with a single essential singularity at infinity in the neighbourhood of that singularity. The term "integral function" is used as a translation of the French expression "*fonction entière*."

Part I opens with an introduction, in which it is pointed out that for each of the few integral functions whose detailed properties have been investigated, there always exists near infinity an asymptotic expansion valid in those parts of the region near infinity which are not at a finite distance from zeros of the function. It is then suggested that wide classes of integral functions admit such expansions; and it is pointed out that, if such a theorem can be proved and the expansions obtained, we may solve many questions relating to the "genre" of a function, to the nature of its zeros, and to the character of its coefficients when expanded as a Taylor's series. We may, in fact, to a large extent classify such functions by their behaviour at infinity.

After the introduction, a short account of the historical development of the enquiry is given; and then the memoir proper commences with a formal arrangement of integral functions according to the laws of distribution of the zeros as seen in expressions in Weierstrassian-product form.

When the n th zero a_n is such that it depends solely upon n and certain definite constants, and also such that the law of dependence is the same for all zeros, we call it a simple integral function with a single sequence of non-repeated zeros, or sometimes briefly a simple integral function.

The zeros are said to be algebraic, when they are given, when n is large, by a formula of the form

$$a_n = c_0 n^\rho \left[1 + \frac{c_1}{n^{\rho_1}} + \frac{c_2}{n^{\rho_2}} + \dots \right],$$

where ρ is a rational positive quantity, and $\rho_1, \rho_2, \dots$ are rational positive quantities arranged in ascending order of magnitude. If ρ is such that $\sum_{n=1}^{\infty} \frac{1}{|a_n|^{\rho+\epsilon}}$ converges, and $\sum_{n=1}^{\infty} \frac{1}{|a_n|^{\rho-\epsilon}}$ diverges, however small the positive quantity ϵ may be, the function is said to be of order ρ .

We can form a scale of simple integral functions arranged according

to the dependence of a_n upon n . When a_n is, for n large, to the first approximation a transcendental function of n , the integral function said to be one of transcendental sequence.

A function with a finite number of simple sequences of zeros can be built up of a number of non-repeated functions each of simple sequence.

When the n th zero of a function of simple sequence is repeated number of times dependent upon n , we call it a repeated integral function.

The typical zero may require more than a single number to define its position in the series of zeros to which it belongs; the integral function is then called one of multiple sequence.

Finally, it is pointed out that the category to which a typical series belongs may be indefinitely complex, and that, therefore, we cannot expect to lay down no general law relating to all integral functions which is not a disguised truism. In the memoir we confine ourselves substantially to simple functions with repeated or non-repeated sequences of zeros.

Part II contains the theory of divergent and asymptotic series. Poincaré's arithmetic definition is first given; the function $J(z)$ admits the asymptotic expansion

$$a_0 + \frac{a_1}{z} + \dots + \frac{a_n}{z^n} + \dots,$$

in certain regions where $|z|$ is very large, if the sum of the first n terms be s_n , and the expression

$$|z^{n-1}(J - s_n)|$$

tends to zero as z tends to infinity in those regions.

The difficulty of this theory is pointed out, and it is shown that the theory dependent on analytic functionality is more tractable and is less precise.

A series

$$a_0 + a_1 z + \dots + a_n z^n + \dots$$

of finite radius of convergence can, by an extension of a method due to Borel, be interpreted by means of a contour integral for all values of the variable outside the circle of convergence, except those which lie along the straight line joining the singularities to infinity, as proceeding directly away from the origin, provided all the singularities of the function represented by the series lie on such a line. The integral is called the "sum" of the divergent series. The process of summation is not unique, but must always lead to the same result, namely, to the function which is the analytic continuation of the function represented by the given series when convergent. On the

other hand, the series for those values of z for which it is divergent is not the arithmetically asymptotic expansion of the function.

At this stage it is natural to invent some extension of the ideas thus employed, and to apply that extension to the case in which the series has zero radius of convergence. It is shown that this new problem is radically different from the old one, the number of solutions being infinite. The solutions given by the process of integration are, however, functions which have zero for their essential singularity, and which admit the given series as an arithmetically asymptotic expansion in certain regions whose apex is that singularity.

We classify divergent series according to their order; a series is said to be of order k when $\frac{\sqrt[n]{a_n}}{n^k}$ is finite and not zero.

Series of the first order can be "summed" by the same process as that employed for series of finite radius of convergence. Series of higher order can be "summed" either by successive repetition of this process, or by means of auxiliary functions derived from the higher hypergeometric functions.

An application is made to the Maclaurin sum formula, and it is shown that this formula may be used to give an asymptotic expansion for $\sum_{n=1}^{m-1} \phi(n)$, when m is large, when $\phi(z)$ is an integral function of z of order greater than or equal to unity.

Finally, the rearrangement of asymptotic series is considered, and Part II closes with a theoretical account of the possibility and nature of the asymptotic expansion of an integral function near its essential singularity.

In Part III actual asymptotic expansions of wide classes of simple integral functions with non-repeated zeros are obtained.

The three standard functions employed are—

$$P_\rho(z) = \prod_{n=1}^{\infty} \left[1 + \frac{z}{n^\rho} \right], \text{ where } \rho > 1;$$

$$Q_\rho(z) = \prod_{n=1}^{\infty} \left[\left(1 + \frac{z}{n^{1/\rho}} \right) e^{-\frac{z}{n^{1/\rho}} + \dots + \frac{(-z)^\rho}{\rho n^{1/\rho}}} \right]$$

where ρ is greater than unity and not integral, and p is an integer such that $p + 1 > \rho > p$;

$$R_\rho(z) = \prod_{n=1}^{\infty} \left[\left(1 + \frac{z}{n^{1/\rho}} \right) e^{-\frac{z}{n^{1/\rho}} + \dots + \frac{(-z)^\rho}{\rho n^{1/\rho}}} \right],$$

where ρ is an integer > 1 .

It is shown that, asymptotically near $z = \infty$, we have

$$\log P_{\rho}(z) = \frac{\pi}{\sin \frac{\pi}{\rho}} z^{1/\rho} - \rho \log \sqrt{2\pi} - \frac{1}{2} \log z + \sum_{s=1}^{\infty} \frac{(-)^{s-1} F(\rho s)}{sz^s};$$

$$\log Q_{\rho}(z) = \frac{\pi}{\sin \pi \rho} z^{\rho} - \frac{1}{2\rho} \log 2\pi - \frac{1}{2} \log z + \sum_{s=-\rho}^{\infty} \frac{(-)^{s-1} F(s/\rho)}{sz^s};$$

and

$$\begin{aligned} \log R_{\rho}(z) = & (-z)^{\rho} \log z + (-)^{\rho-1} \frac{z^{\rho}}{\rho} - \frac{1}{2} \log z - \frac{1}{2\rho} \log 2\pi \\ & + \sum_{s=-\rho}^{\infty} \frac{(-)^{s-1} F(s/\rho)}{sz^s} \end{aligned}$$

where the numbers F are substantially Riemann ζ -functions of ρ and $1/\rho$ as the argument.

It is then shown that these expansions can be generalised so as to apply to wide classes of functions, notably those with algebraic sequence of zeros. General formulæ are given which symbolically contain the expansion of all integral functions of the type considered, which are such that the Maclaurin sum formula can be applied to the function expressing the dependence of the n th zero upon n . An example of a function with transcendental sequence, the asymptotic expansion of

$$\prod_{n=1}^{\infty} \left[1 + \frac{z}{e^n} \right]$$

is obtained completely.

Part IV deals with the asymptotic expansion of repeated integral functions of simple sequence. It is necessary to take an extended definition of "order" in the case of such functions, and then the expansions are obtained for standard functions—

- (1.) Of finite (non-zero or zero) order less than unity;
- (2.) Of finite non-integral order greater than unity;
- (3.) Of finite integral order greater than or equal to unity.

Subsequently symbolic formulæ are given for all integral functions of the prescribed type; and, as an example, the asymptotic expansion of a repeated function with a transcendental index is considered. The formulæ are verified in the case of the G -function.

Part V is devoted to applications of the previous expansions to the problems mentioned in the introduction.

A knowledge of the asymptotic expansion of a function serves to determine the number of roots which it possesses inside a circle of given large radius. If the function is of order ρ , the number of roots within a circle of large radius r is to a first approximation

$$\frac{\sin \pi \rho}{\pi} \log \phi(r),$$

where $\phi(r)$ is the maximum value of the function on the circle in question. When ρ is integral, this approximation becomes

$$\frac{\log \phi(r)}{\log r}.$$

A number of theorems are given relating to the order of functions combined from functions of given order.

It is shown that a function is of the same order as its derivative, and that from the knowledge of the zeros of a function we may derive an approximation to the large zeros of its derivative.

If a function is of "genre" p , its derivative can at most have only p zeros beyond those given by the extension of Rolle's theorem.

The memoir closes by suggesting other applications of the theory.

"On Areal Induction." By GEORGE J. BURCH, M.A., F.R.S.,
Reading College, Reading. Received September 10, 1901.

In my paper "On the Relation of Artificial Colour-blindness to Successive Contrast,"* I described a modification of some experiments by Sigmund Exner.

A disc, half black and half white, with a very narrow slit at the junction of the two halves, revolves in front of an incandescent lamp. Another lamp throws a light upon the face of the disc. While it is rotating slowly, the incandescent filament of the lamp, seen through the slit as it passes across, looks bright against the white card, but when a certain velocity is reached it appears as a black thread against a brighter background.

I suggested as the explanation of this phenomenon that the intense light of the filament induced an amount of fatigue out of all proportion to the sensation excited by it, so that the less fatiguing illumination of the white card produced a greater effect on the senses than the sum of the sensations due to the filament and the subsequent light.

In a recent paper,† however, Dr. Shelford Bidwell has described an experiment having an important bearing on the subject.

By a suitable arrangement a disc of green light is projected on a screen; this is followed by a somewhat larger disc of white light, and the cycle is completed by an interval of darkness. The novelty of the experiment consists in the addition of a black spot in the centre of the white disc, so that when the apparatus is worked the site of this spot is illuminated five or six times a second by green light only,

* 'Roy. Soc. Proc.,' vol. 66, p. 212.

† 'Roy. Soc. Proc.,' vol. 68, p. 277.

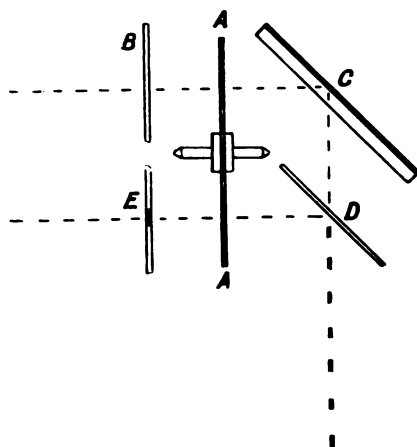
whereas on the surrounding portions each green flash is followed by a white one. The area corresponding to the green disc appears with a black spot in the middle.

Obviously this experiment falls under the first of the four classes of successive contrast described in my paper,* *i.e.*, the first excites a single colour-sensation, and the second excites all the other sensations. The conditions are less simple than those of the second or fourth classes described by me, and I have therefore named this Dr. Shelford Bidwell's experiment, and have extended the principle to the other classes of contrast referred to.

The following arrangement serves admirably for exhibiting the phenomenon. In front of the disc A is placed a piece of coloured glass (preferably green or blue) B, and behind it, at an angle to its plane, the silvered glass mirror C. Light passing through B is reflected from C parallel to the plane of the disc. In the path of the reflected rays is a second mirror, D, of unsilvered glass, so placed that it reflects rays from E in the same direction. E may conveniently be a black bead fixed on a wire or a black spot on a piece of glass.

The disc A, fig. 1, has two opposite sectors of about 30° each, and the positions of B and E are so regulated that on rotating the disc a flash of coloured light from B is seen first in the mirror, and

FIG. 1.



flash of white light of less intensity is reflected from D, and in the midst of it the image of the black bead, this being followed by a period of darkness. As long as the rate is not much more than 10 revolutions per second the light, when green glass is used, is

* 'Roy. Soc. Proc.' vol. 66, p. 287.

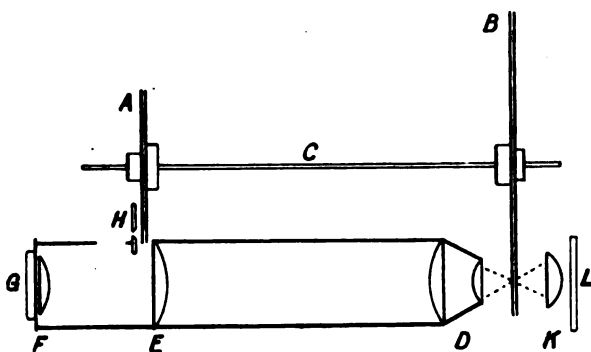
, and the bead looks quite black against it. But when the disc is turned faster, at a certain speed both bead and glass look green, and when the speed is diminished again, the bead looks black and purple again respectively when the speed diminishes. When the intensity of the flash is reduced, there comes a point at which the phenomenon ceases to be observed, and if the area of the spot is increased, a limit is reached beyond which the effect no longer extends from the centre. My results agree as to these details completely with those obtained by Dr. Shelford Bidwell.

The essential difference between this experiment and those described in my previous paper consists in the use of the black spot, and the fact described by Dr. Shelford Bidwell, that the contrast effect may be so great in a spot not directly subjected to the first stimulus, is of the greatest importance, proving, as he points out, that the first sensation is merely swamped by the second, but actually never comes into existence.

Dr. Bidwell, however, agrees with him in thinking that no explanation can be afforded by the Young-Helmholtz theory, nor do I consider it warrants us in postulating an independent white sensation, and this is supported by the following experiments, made on the lines laid down in my previous paper.*

Discs A and B, fig. 2, are carried by a shaft C about 100 cm. Each disc is composed of two equal circular sheets of metal, each pair of sectors of 30° cut out at opposite ends of a diameter, so

FIG. 2.



rotating one over the other the aperture may be of any desired size and fitted with clamping nuts so that the relative position of disc B on the shaft can be varied. D is an objective of 7 cm. focal length, and E is a plano-convex lens 10 cm. in diameter, which with the smaller lens F forms in effect a Campani eye-piece of about

* 'Roy. Soc. Proc.,' vol. 66, p. 209.

12.5 cm. focal length. G is a Thorp's replica of Rowland's gratings 15,000 lines to the inch, giving a spectrum of the slit H apparently in the centre of the field of view of the optical system formed by the lenses D, E, F, which constitutes a kind of microscope, with the lens K serving as condenser. Behind K is placed a screen L of suitable coloured glass, or a train of prisms for illuminating the apparatus with spectral light.

On rotating the shaft the disc A allows a narrow spectrum of about 10° dispersion and 1° wide to appear momentarily, and immediately afterwards a bright flash of monochromatic light fills the field.

As in my paper on Simultaneous Contrast,* a strip of black material is pasted on the plane side of the lens E to serve as a background for the spectrum, which should exactly cover it. The part therefore, of the retina on which the image of the spectrum falls is not directly stimulated by the second flash, which fills the rest of the field. The results show, however, that it is indirectly affected as in Dr. Shelford Bidwell's experiments.

With a screen of ruby glass the spectrum corresponds with that seen during temporary red blindness, i.e., the red is missing, and the orange and yellow are replaced by green. With a blue screen composed of film stained with Prussian blue, and a pale cobalt, or, better, a gelatin film stained with aniline blue, the blue and violet disappear, and the green is weakened, but the red is bright. The addition to these of yellow glass causes the violet of the spectrum to reappear. And the same effect is produced by a green glass. In a word, the results are exactly similar to those described in Section II, paragraph 3, of my paper on Successive Contrast, the difference between the two experiments being that in the new one the retinal area on which the contrast effect, or colour-blindness, is produced, not being exposed to the direct action of the second stimulus, it becomes possible to use a series of rapid flashes instead of a single one, and the effect appears practically continuous.

But the difference between this and Dr. Shelford Bidwell's experiment consists in the use by him of a second stimulus compounded of all the colours, and by me of a second stimulus restricted to one colour. The first step in the discussion must be to ascertain the relation between the results obtained when the second stimulus is white, and when the second stimulus is of the same colour as the first.

In every experiment I have made, whether with one double flash or with a series of such flashes, the result has been the production of a considerable degree of momentary blindness to the colour or colours composing the second flash of the cycle. And this statement is equally true if the two flashes are of different colours. Whatever colour sensations are common to the two flashes are wholly or partly blotted

* 'Roy. Soc. Proc.' vol. 67, p. 227.

out, and the remainder are combined in the resulting sensation. But inasmuch as the optimum period of the cycle is different for each colour-sensation, the use of pigment colours leads sometimes to apparently divergent results, which, however, are easily accounted for when the spectra of the pigments are taken into account.

In manipulating the apparatus when both flashes are monochromatic a farther clue to the explanation is obtained. To produce the maximum Exner-effect, the second stimulus must be much less intense than the first, though it may last longer. This accounts for the difference of the appearance when the direction of the rotation is reversed, since, unless the relative intensity and duration as well as the colours of the flashes are interchanged, the conditions under which the phenomenon occurs are no longer observed.

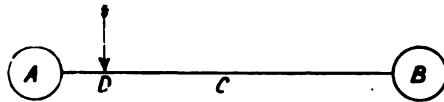
I fail to see why an independent white sensation must be postulated. If the first stimulus is green and the second white, then, on Young's hypothesis, the second stimulus consists of red, green, violet (and blue according to my own experiments). Accordingly, the green, which is common to both stimuli, vanishes, and the eye perceives that mixture of red, blue, and violet so familiar in purple pigments and flowers. It appears to me that Dr. Shelford Bidwell's black-spot experiment adds farther confirmation to this explanation, since it shows conclusively that the response to the first stimulus is inhibited—if that word may be borrowed—by a second stimulus falling upon a neighbouring part of the retina. For the second stimulus, white, must necessarily include the same physical stimulus, green, as the first, and this component of it is by itself, as my experiments show, quite competent to produce the observed result, namely, the inhibition of the response to the first stimulus. Moreover, a second stimulus which contains all the elements of white save green does not inhibit the response to green, and the same is true of the other colours. If the first stimulus is red and the second blue, or if the first is green and the second purple, or *vice versa*, i.e., if neither stimulus includes any of the components of the other, the effect is nil. It is difficult to see why this should be the case if the phenomenon is a function of a white sensation. To me it seems more simple to explain the whole phenomenon as follows :—

It is granted that a diminution of the intensity of a sensation occurs whenever the stimulus is prolonged. This diminution may occur so suddenly under suitable conditions that it becomes difficult to regard it as the mere result of using up material previously stored. It resembles rather those reflexes by which the organs of vision are protected from a sudden light, namely, the winking of the eyelids and the contraction of the iris, and is probably a provision for preventing unnecessary waste of material.

The question then arises whether such a function would be located

in the central or in the peripheral organ. Let A, fig. 3, represent a peripheral organ, B a central organ, and C the nerve connecting them. Let a stimulus, E, applied at A evoke a sensation, S, at B. Similarly, let another stimulus, E_2 , applied at A evoke a corresponding sensation, S_2 , at B. Then the experimental facts may be stated thus: if there is a certain time-interval of $\frac{1}{70}$ of a second between E_1 and E_2 , and a certain relation between them of intensity and duration, the sum of the resulting sensations S_1 and S_2 may be practically zero, or, at any rate, very much smaller than either sensation by itself.

FIG. 3.



(1.) The first obvious deduction is that made by Exner, viz., that period of at least $\frac{1}{70}$ " must elapse between the application of the stimulus at A and the arousing of the sensation at B.

(2.) It is also evident that this delay of $\frac{1}{70}$ " is not simply the transmission time of the nerve-response along C from A to B, for, if were, the second impulse would reach B too late to affect the first unless we suppose either a double track along C or the propagation along a single track of a double response, with components travelling at different rates—a hypothesis not supported by any facts within our knowledge.

(3.) If the delay of $\frac{1}{70}$ " occurs at B we must also suppose either a double track along C, or else that each nerve-fibre is capable of transmitting two opposite kinds of response, namely, one with latency of $\frac{1}{70}$ " arousing sensation, and the other deadening it, and acting without delay.

(4.) It seems simpler to suppose that this delay of $\frac{1}{70}$ " occurs somewhere on the track between A and B, and probably close to A, e.g., at D, and that it is quite distinct from any delay due to transmission.

(5.) Not only do these experiments prove that there is a delay, but that, under certain circumstances, a second stimulus may modify the point of annulling it the response to a previous stimulus. It has been shown* that two stimuli in rapid succession may give rise in a nerve to what appears to be a single response, but in that case the response is at least as strong as either stimulus would produce alone, whereas here the response is less than it would have been without the second stimulus.

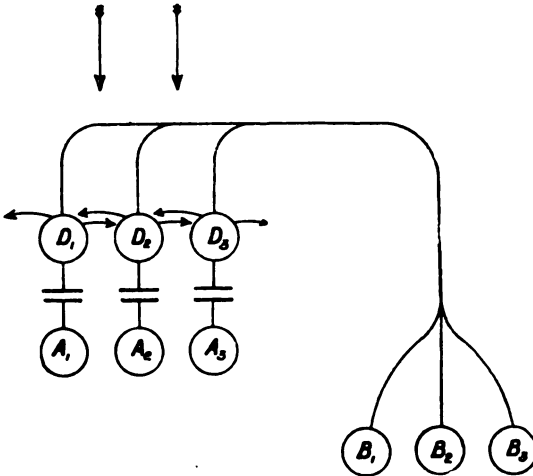
These facts point to the existence, probably in the retina, of some mechanism sensitive to light, the function of which is to regulate for

* Gotch and Burch, 'Journal of Physiology,' vol. 24, p. 410.

ment of the visual field the intensity of the visual impulse 1 to the central organ, much as the iris and the eyelids for the whole. It is acknowledged that we cannot as yet definite functions to the several elements of the retina, and a not demonstrate a structural continuity between the three hich it is composed, although we may conclude that changes n the rods and cones sweep through the inner nuclear layer along the fibres of the optic nerve as nervous impulses.* herefore, room between the extreme peripheral structures eginnings of the optic nerve, for a structure, which when by light falling on it may block wholly or in part the im- ing to it from the periphery.

three similar hypothetical elements of the retina are drawn side by side. In each A represents the sensitive peripheral giving rise, when stimulated by light, to a visual impulse, and

FIG. 4.



its the controlling structure, also sensitive to light, the func- ich is to block the visual impulse if necessary. D_1 is repre- having cross-connections with D_2 , and *vice versa*. Suppose light, F_1 , coming in the direction of the arrow to fall sud- ll three retinal elements. It gives rise in A_1 , A_2 , and A_3 to n of excitation—lasting longer than the flash itself, and lually away, arousing in the central organ the sensation of ng its earlier and of the “positive after-effect” during its s. But at the same time D_1 , D_2 , and D_3 are excited, and

* Foster, 'Text-book of Physiology,' Part IV, p. 1203.

they exercise a momentary control over the visual impulses from A_1 , A_2 , and A_3 , transmitting them through C to B with an intensity considerably reduced at first in the case of a bright flash. Perhaps even the apparent delay of $\frac{1}{75}$ " may be due to this initial reduction.

Next suppose a second flash falling on $A_1 D_1$ and $A_3 D_3$. Owing to the cross connections from D_1 and D_3 to D_2 , the activity of D_2 is aroused again, and the visual impulse from A_2 is blocked once more, so that little or none of it gets through to B_2 .

If we suppose with Young that each colour-sensation results from the activity of an independent peripheral system, it is easy to explain the experimental phenomena. Let the second stimulus be white and the first green. D_1 and D_3 , having been made to act strongly by the repeated excitation, will block all visual impulses of green from A_1 and A_3 by direct action, and from A_2 by virtue of their cross-connection with D_2 . The result will be that the components of purple, i.e. $(W-G) = \{(R+G+B+V) - G\}$ will reach B_1 and B_3 , and nothing at all will reach B_2 —exactly as in Dr. Shelford Bidwell's experiment.

It is evident from a multitude of experiments that we have to do with two classes of contrast effects, namely, those which are induced gradually and pass off slowly, and others which are momentary and suggest the term spasmodic. The latter require for their production a sharp and sudden stimulus, and one such stimulus may even be followed by a multiple response, as in the recurrent images of Purkinje or Charpentier's bands. To myself these phenomena recur very forcibly the multiple electrical response of a nerve-muscle preparation, or of an electrical organ, to a single strong stimulus, and I suggest that the "recurrent image" is caused by intermittent blocking of the positive after-effect. The failure of Talbot's law for low speeds is easily explained on this hypothesis. The ratio of the magnitude of the response of the "D" structures to the magnitude of the stimulus is greater when the stimulus is repeated a certain number of times per second, i.e., a larger percentage of the total visual impulse from A is blocked by D when there are fifty flashes per second than when there are only five, and consequently the mean brilliancy appears less until the limit is reached, beyond which Talbot's law holds.

The phenomenon is manifested in greatest intensity when the stimulus consists of two or three flashes close together followed by an interval of rest. Possibly the reason of this is partly that the activity of D may be maintained, and partly that A may recover from its fatigue; however that may be, it is essential in all experiments of the reversal of the image that the eye should be in darkness for one-half of the cycle.

Another phenomenon explicable on this hypothesis is the flickering of certain spectral colours. If a small weight is held out at arm's length, after a time the muscles begin to shake and quiver. And if

highly magnified spectrum is gazed at steadily in the neighbourhood of D, or F, or G, the colours begin to change and flicker exactly as if some controlling mechanism were getting fatigued. But if either of the contending sensations is *thoroughly* exhausted, as in artificial colour-blindness, this flickering ceases, and is abolished until the retina recovers. I have called attention to the fact that the positive after-effect or dazzle-tint of artificial colour-blindness does not undergo those cyclic changes of colour which have been so long familiar in ordinary after-images.*

To sum up—

(1.) There appears to be strong evidence of the existence in the retina, between the structures in which the visual impulses originate and the beginnings of the optic nerve, of a set of structures by which the intensity of the visual impulses transmitted to the central organ is regulated.

(2.) That these structures are sensitive to light, and when excited block wholly or in part visual impulses coming from the periphery.

(3.) That these structures are cross-connected so that the excitation of one affects those in its neighbourhood, thus increasing the contrast between a bright object and its surroundings, and probably preventing the blurring of an image by diffused light from the retina.†

(4.) That they may by sudden stimuli be excited to a very powerful or even a multiple response.

(5.) That many phenomena of contrast and of areal induction may be accounted for on such a hypothesis.

(6.) That Dr. Shelford Bidwell's experiments are explicable without assuming a white sensation.

"Further Observations on Nova Persei. No. 4." By Sir NORMAN LOCKYER, K.C.B., F.R.S. Received October 10, 1901.

The last paper‡ contained an account of the observations of the Nova made at Kensington between March 25 and May 7. The observations are, in the present paper, brought up to September 30. Between June 18 and August 8 no work was done on the Nova, owing chiefly to the interference of buildings and twilight.

* 'Phil. Trans.,' B, vol. 191, p. 7.

† I find no evidence of any such cross-connection between one colour sensation and another.

‡ 'Roy. Soc. Proc.,' vol. 68, p. 399.

Magnitude.

The magnitude has been estimated on sixteen evenings. These seem to afford sufficient evidence that since the beginning of May the star has decreased in magnitude from about 5·7 to 6·7; further, the short variations previously recorded apparently no longer exist, as the estimated magnitudes on eleven nights since the end of August only vary between 6·5 and 6·8.

Photographs of the region about the Nova were taken with the 6-inch Dallmeyer doublet on the evenings of August 25 and September 11, 12, and 18. From these it has been ascertained that its magnitude relatively to those of the surrounding stars is greater photographically than visually.

Visual Spectrum.

The spectrum has been observed with a McClean direct-vision spectroscope on the evenings of August 8, 13, 17, 20, 22, 24, and September 11, 12, 18. The line in the position of the chief nebular line at λ 5007 has always been noted as being the strongest, $H\beta$ and the line at λ 4959 being also present, but comparatively much weaker. Fairly bright lines near λ 472 and the region of $H\gamma$ have also been recorded. Traces of a few lines less refrangible than λ 5007 have been glimpsed, but no determinations of their wave-lengths have been made. No lines have been observed in the yellow or red regions of the spectrum.

Photographic Spectrum.

Photographs have been taken on the evenings of August 9, 15, 17, 21, 22, 23, 24, 26, September 3, 5, 9, 10, 11, 12, 18, 21, 23, 25, 26, with the 9-inch prismatic reflector, the Nova being now far too faint for the utilisation of the 6-inch objective prism, and, until quite recently, in such a position as to make the 30-inch reflector unavailable. These photographs show little or no variation amongst themselves either in the number, position, or relative intensities of the lines. The scale of the photographs is such that the distance between $H\beta$ and $H\epsilon$ is about 0·9 cm. The wave-lengths of the lines have been determined from the best photograph (August 26) by means of an interpolation curve. The presence in the Nova spectrum of some of the hydrogen lines and the helium line λ 4471 was established by direct comparison with the spectra of other stars photographed with the same instrument. These lines were used, together with the 5007 line (the identity of which with the strongest nebula line was assumed), as fiducial lines for the construction of the interpolation curve. The remaining lines were then measured and their wave-lengths read off from the curve. The intensities of the lines as they appear on the plate were also noted.

The spectrum has undergone great modification since the last photographs were taken at Kensington in March and April. The lines are about the same in number and position, but their nature and relative intensities have vastly changed. In the latter part of March and the early part of April the lines were generally broad and ill-defined. They are now narrower, and with better defined edges. The greatest change noticeable in the spectrum is with respect to the relative weakening of the hydrogen lines generally and the alteration in relative intensity amongst some of them. In the earlier photographs the hydrogen lines were very prominent, and the individual members of the series gradually weakened as the ultra-violet part of the spectrum was approached. In the latest photographs they appear of quite secondary importance. $H\beta$, $H\gamma$, and $H\delta$ are all present, but occur only as comparatively weak lines, especially the latter two. With regard to $H\epsilon$, apparently the only other hydrogen line shown, the case is different. The line at or near the position of $H\epsilon$ is now about the second strongest line in the whole photographic spectrum, and many times stronger than either $H\delta$, $H\gamma$, or $H\beta$. With this fact in view, it may be suggested that this strong line is possibly not wholly due to hydrogen, but is mainly a new line of unknown origin, which happens to fall exactly on, or very near to, the position of $H\epsilon$. The line appears to be of much the same nature as the stronger adjacent line at λ 3868. The following table gives the wave-lengths and intensities of the lines visible in the photographs:—

$\lambda\lambda$.	Int. (max. = 10).	Remarks.
3968	10	Strong line in spectrum of planetary nebulae.
3970	8	Probably not wholly due to hydrogen ($H\epsilon$), intensity abnormal.
4102	2	$H\delta$.
4341	2	$H\gamma$.
4364	7	
4471	1	Helium.
4636	3	
4684	2	Probably identical with 4687 of ζ Puppis and Orion stars.
4720	6	
4808	<1	
4862	3	$H\beta$.
4959	2	Apparently identical with second nebula line.
5007	4	" " strongest "

In addition to the lines in the above table, six of the photographs, including that of August 26, give indications of the presence of a line in the extreme ultra-violet. Its wave-length could not be obtained

with any degree of accuracy owing to lack of comparison photograph showing lines in the same part of the spectrum, but it is probably identical with the line at λ 342 which von Gothard has independently recorded.*

The enhanced lines of iron, magnesium, &c., which were such a conspicuous feature of the earlier photographs of the Nova spectrum, have now entirely disappeared, and the probability is that the bright line of the present spectrum other than those of hydrogen and helium belong to gases the terrestrial equivalents of which have not been found. It may be mentioned, however, that in their paper "On the Spectrum of the more Volatile Gases of Air,"† Professors Living and Dewar record weak lines at $\lambda\lambda$ $\begin{smallmatrix} 4363 \\ 4365 \end{smallmatrix}$ and 4636 which may possibly have the same chemical origin as the Nova lines 4364 and 4636.

As there is no indication of the new lines forming a series similar to those given by hydrogen and helium, there is probably more than one gas involved.

Characteristics of the Hydrogen and other Lines.

In the series of photographs under discussion, the hydrogen lines no longer exhibit the curious structure described in previous communications, but are much fainter and rather narrower and devoid of any apparent differences in density. Some of the bright lines—notably λ 3868 and λ 3970—are of about the same width as the adjacent hydrogen lines were in the earlier photographs, and show a structure somewhat similar to that illustrated in a previous note.‡

The lines in question appear as triplets, and in each case the more refrangible member is slightly brighter than the others. The lines λ 4364 and λ 4720 are narrower and less intense than those just mentioned, and put on the appearance of doubles, the brighter member being still on the more refrangible side.

Appearance of the Star.

Visual examination of the star under high powers shows that the Nova has a decidedly larger disc than is possessed by neighbouring stars of similar magnitude. Photographs of the region of the Nova taken on the nights of September 11 and 12, 1901, with a Dallmeyer rectilinear objective of 6 inches aperture and focal length of 4 feet 6 inches, show the penumbral aureole very clearly. It has a fairly definite outline, and is altogether different in character from the

* 'Astro. Nach.,' No. 3738.

† 'Roy. Soc. Proc.' vol. 67, p. 467.

‡ 'Roy. Soc. Proc.' vol. 68, p. 234.

images of neighbouring stars of equal magnitude. This peculiarity of the Nova's disc was first remarked by Flammarion and Antoniadi, and later by Max Wolf, Kostinsky, and von Gothard. It owes its origin probably to the exceptionally strong ultra-violet rays emitted by the Nova, which are not brought to the focus for which the objective is corrected.

The recent photographs have been taken by Mr. Butler and Mr. Rolston. The visual observations have chiefly been made by Messrs. Fowler and Butler. Mr. Baxandall has undertaken the reduction to wave-lengths and the discussion of the lines in the photographic spectrum, while Dr. Lockyer and Mr. Baxandall have assisted in the preparation of the present paper.

"An Attempt to ascertain the Date of the Original Construction of Stonehenge from its Orientation." By Sir NORMAN LOCKYER, K.C.B., F.R.S., and F. C. PENROSE, F.R.S. Received October 21, 1901.

This investigation was undertaken in the spring of the present year, as a sequel to analogous work in Egypt and Greece, with a view to determine whether the orientation theory could throw any light upon the date of the foundation of Stonehenge, concerning which authorities vary in their estimate by some thousands of years. We beg to lay before the Royal Society the results derived from a careful study of its orientation for the purpose of arriving at the probable date of its foundation astronomically. This is not, indeed, the first attempt to obtain the date of Stonehenge by means of astronomical considerations. In Mr. Godfrey Higgins' work* he refers to a method of attack connected with precession. This furnished him with the date 4000 B.C.

More recently, Dr. W. M. Flinders Petrie,† whose accurate plan is a valuable contribution to the study of Stonehenge, was led by his measures of the orientation to a date very greatly in the opposite direction, but, owing to an error in his application of the change of obliquity, clearly a mistaken one.

As the whole of the argument which follows rests upon the assumption of Stonehenge having been a solar temple, a short discussion of the grounds of this view may not be out of place; and, again, as the approximate date which we have arrived at is an early one, a few words may be added indicating the presence in Britain at that time of a race of men capable of designing and executing such work.

* 'The Celtic Druids.' 4to. London, 1827.

† 'Stonehenge,' &c., 1880.

As to the first point, Diodorus Siculus (ii, 47) has preserved a statement of Hecataeus in which Stonehenge alone can by any probability be referred to.

"We think that no one will consider it foreign to our subject to say a word respecting the Hyperboreans.

"Amongst the writers who have occupied themselves with the mythology of the ancients, Hecataeus and some others tell us that opposite the land of the Celts [*ἐν τοῖς ἀντιπέραν τῆς κελτικῆς τόποις*] there exists in the Ocean an island not smaller than Sicily, and which, situated under the constellation of The Bear, is inhabited by the Hyperboreans; so called because they live beyond the point from which the North wind blows. . . . If one may believe the same mythology, Latona was born in this island, and for that reason the inhabitants honour Apollo more than any other deity. A sacred enclosure [*νῆρον*] is dedicated to him in the island, as well as a magnificent circular temple adorned with many rich offerings. . . . The Hyperboreans are in general very friendly to the Greeks."

The Hecataeus above referred to was probably Hecataeus of Abdera, in Thrace, fourth century B.C.; a friend of Alexander the Great. This Hecataeus is said to have written a history of the Hyperboreans: that it was Hecataeus of Miletus, an historian of the sixth century B.C., is less likely.

As to the second point, although we cannot go so far back in evidence of the power and civilisation of the Britons, there is an argument of some value to be drawn from the fine character of the coinage issued by British kings early in the second century B.C., and from the statement of Julius Cæsar ('*De Bello Gallico*,' vi, p. 13) that in the schools of the Druids the subjects taught included the movements of the stars, the size of the earth and the nature of things (*Multa præterea de sideribus et eorum motu, de mundi magnitudine, de rerum natura, de deorum immortalium vi ac potestate disputant et juventuti tradunt*).

Studies of such a character seem quite consistent with, and to demand, a long antecedent period of civilisation.

The chief evidence lies in the fact that an "avenue," as it is called, formed by two ancient earthen banks, extends for a considerable distance from the structure, in the general direction of the sunrise at the summer solstice, precisely in the same way as in Egypt a long avenue of sphinxes indicates the principal outlook of a temple.

These earthen banks defining the avenue do not exist alone. As will be seen from the plan which accompanies this paper, there is a general common line of direction for the avenue and the principal axis of the structure, and the general design of the building, together with the position and shape of the Naos, indicate a close connection of the *whole temple structure* with the direction of the avenue. There may have been other pylon and screen equivalents as in ancient temples.

which have disappeared, the object being to confine the illumination to a small part of the Naos. There can be little doubt, also, that the temple was originally roofed in, and that the Sun's first ray, suddenly admitted into the darkness, formed a fundamental part of the cultus.

While the actual observation of sunrise was doubtless made within the building itself, we seem justified in taking the orientation of the axis to be the same as that of the avenue, and since in the present state of the S.W. trilithon the direction of the avenue can probably be determined with greater accuracy than that of the temple axis itself, the estimate of date in this paper is based upon the orientation of the avenue. Further evidence will be given, however, to show that the direction of the axis of the temple, so far as it can now be determined, is sufficiently accordant with the direction of the avenue.

The orientation of this avenue may be examined upon the same principles that have been found successful in the case of Greek and Egyptian temples—that is, on the assumption that Stonehenge was a solar temple, and that the greatest function took place at sunrise on the longest day of the year. This not only had a religious motive; it had also the economic value of marking officially and distinctly that time of the year and the beginning of an annual period.

It is, indeed, probable that the structure may have had other capabilities, such as being connected with the equinoxes or the winter solstice; but it is with its uses at the summer solstice alone that this paper deals.

There is this difference in treatment between the observations required for Stonehenge and those which are available for Greek or Egyptian solar temples—viz., that in the case of the latter the effect of the precession of the equinoxes upon the stars, which as warning clock stars were almost invariably connected with those temples, offers the best measure of the dates of foundation; but here, owing to the brightness of twilight at the summer solstice, such a star could not have been employed, so that we can rely only on the secular changes in the obliquity as affecting the azimuth of the point of sunrise. This requires the measurements to be taken with very great precision, towards which care has not been wanting in regard to those which we submit to the Society.

The main architecture of Stonehenge consisted of an external circle about 100 feet in diameter, composed of thirty large upright stones, termed sarsens, connected by continuous lintels, and an inner structure of ten still larger stones, arranged in the shape of a horseshoe, formed by five isolated trilithons. About one-half of these uprights have fallen and a still greater number of the lintels which they originally carried. There are also other lines of smaller upright stones, respecting which the only point requiring notice in this paper is that none of them could have interrupted the line of the axis of the avenue. This

circular temple was also surrounded by an earthen bank, also circular, of about 300 feet in diameter, interrupted towards the north-east, receiving into itself the banks forming the avenue before mentioned, which is about 50 feet across. Within this avenue, and, looking east from the centre of the temple, at about 250 feet distance considerably to the right hand of the axis, stands an isolated stone which from a mediæval legend has been named the Friar's Heel.

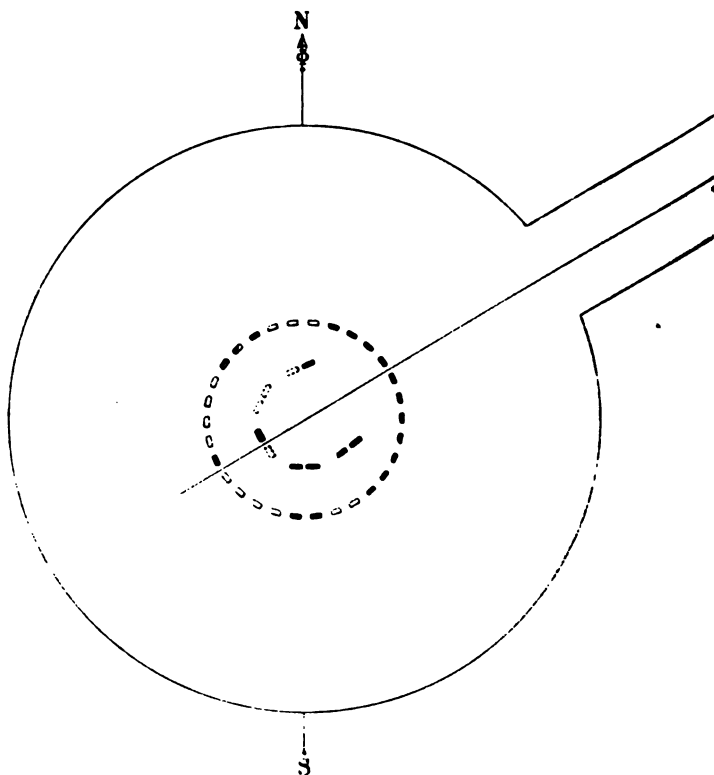


FIG. 1.—General plan of Stonehenge.

The axis passes very nearly centrally through an intercolumn (so to call it) between two uprights of the external circle between the uprights of the westernmost trilithon as it originally stood. Of this trilithon the southernmost upright with the stone fell in the year 1620, but the companion survived: a leaning stone which formed a conspicuous and picturesque object many years, but happily now restored to its original more dignified and safer condition of verticality. The inclination of this stone was *ever, having taken place in the direction of the axis of the*

and as the distance between it and its original companion is known both by the analogy of the two perfect trilithons and by the measure of the mortice holes on the lintel they formerly supported, we obtain by bisection the measure (*viz.* 11 inches) from its edge of a point in



FIG. 2.—The stake placed on the fallen stone indicates the axis shown on Fig. 1, in relation to the leaning stone (on the left), and the centre of the N.E. trilithon.

the continuation of the central axis of the avenue and temple, and which has now to be determined very accurately. The banks which form the avenue have suffered much degradation. It appears from Mr. Richard Colt Hoare's account that at the beginning of the last century they were distinguishable for a much greater distance than at present, but they are still discernible, especially on the northern side,

for more than 1300 feet from the centre of the temple, and particularly the line of the bottom of the ditch from which the earth was taken to form the bank, and which runs parallel to it. Measurements taken from this line assisted materially those taken from the crown of the bank itself. With this help and by using the southern bank and whenever it admitted of recognition, a fair estimate of the centre could be arrived at. To verify this, two pegs were placed at 140 feet apart along the line near the commencement of the avenue and four others at distances averaging 100 feet apart nearer the further recognisable extremity, and their directions were measured with the theodolite, independently by two observers, the reference point being Salisbury Spire, of which the exact bearing from the centre of the temple had been kindly supplied by Colonel John R.E., the Director-General of the Ordnance Survey. The same was also measured locally by observations of the Sun and of Polaris, the mean of which differed by less than 20" from the Ordnance value. The resulting observations gave for the axis of the avenue near the commencement an azimuth of $49^{\circ} 38' 48''$, and for that of the distant part $49^{\circ} 32' 54''$. The mean of these two lines drawn through the central interval of the great trilithon, already referred to, between two of the sarsens of the exterior circle, which have an opening of about 4 feet, within a few inches of their middle point, the deviation being northwards. This may be considered to prove the close coincidence of the original axis of the temple with the direction of the avenue.

This value of the azimuth, the mean of which is $49^{\circ} 35' 51''$, is confirmed by the information, also supplied from the Ordnance Survey, that from the centre of the temple the bearing of the principal landmark on the ancient fortified hill, about 8 miles distant, a well-known British encampment named Silbury or Sidbury, is $49^{\circ} 34' 18''$, and that the same line continued through Stonehenge to the south strikes another ancient fortification, namely, Grovely Castle, 6 miles distant and at practically the same azimuth, viz., $49^{\circ} 35'$. For the above reasons $49^{\circ} 34' 18''$ has been adopted for the azimuth of the avenue.

The present solstitial sunrise was also watched for on five successive mornings, viz., June 21 to 25, and was successfully observed on the latter occasion. As soon as the Sun's limb was sufficiently above the horizon for its bisection to be well measured, it was found to be to the northwards of the peak of the Friar's Heel, which was used as a reference point, the altitude of the horizon being $35' 48''$. The azimuth of this peak from the point of observation had been previously ascertained to be $50^{\circ} 39' 5''$, giving for that of the Sun when measured, $50^{\circ} 30' 25''$, and by calculation that of the Sun when its limb is 2' above the horizon should be $50^{\circ} 30' 54''$. This observation

was therefore completely in accordance with the results which had been obtained otherwise.

The time which would elapse between geometrical sunrise, that is, with the upper limb tangential with the horizon, and that which is here supposed, would occupy about 17 seconds, and the difference of azimuth would be $3' 15''$.

The remaining point is to find out what value should be given to the Sun's declination when it appeared showing itself $2'$ above the horizon, the azimuth being $49^{\circ} 34' 18''$.

The data thus obtained for the derivation of the required epoch are these :—

(1.) The elevation of the local horizon at the sunrise point seen by a man standing between the uprights of the great trilithon (a distance of about 8000 feet) is about $35' 30''$, and $2'$ additional for Sun's upper limb makes $37' 30''$.

(2.) — Refraction + parallax, $27' 20''$.

(3.) Sun's semi-diameter, allowance being made for greater eccentricity than at present, $15' 45''$.

(4.) Sun's azimuth, $49^{\circ} 34' 18''$, and N. latitude, $51^{\circ} 10' 42''$.

From the above data the Sun's declination works out $23^{\circ} 54' 30''$ N., and by Stockwell's tables of the obliquity, which are based upon modern determinations of the elements of the solar system,* the date becomes 1680 B.C.

It is to be understood that on account of the slight uncertainty as to the original line of observation and the very slow rate of change in the obliquity of the ecliptic, the date thus derived may possibly be in error by ± 200 years.

In this investigation the so-called Friar's Heel has been used only as a convenient point for reference and verification in measurement, and no theory has been formed as to its purpose. It is placed at some distance, as before mentioned, to the south of the axis of the avenue, so that at the date arrived at for the erection of the temple the Sun must have completely risen before it was vertically over the summit of the stone. It may be remarked, further, that more than 500 years must yet elapse before such a coincidence can take place at the beginning of sunrise.

We give in an appendix certain details of the observations.

We have to express our thanks to Sir Edmund Antrobus, Bart., for much kind assistance during our survey; and to A. Fowler and Howard Payn, Esqs., for skilful and zealous co-operation in the measurements and calculations. As already stated, Colonel Duncan A. Johnston, R.E., Director-General of the Ordnance Survey, has also

* 'Smithsonian Contributions to Knowledge,' vol. 18, No. 232, table 9. Washington, 1878.

been good enough to furnish us with much valuable information, which our best thanks are due.

APPENDIX.

The instrument chiefly employed was a 6-inch transit theodolite by Cooke with verniers reading to 20" in altitude and azimuth. Most of the observations were made at two points very near the axis, which may be designated by *a*, *b*. Station *a* was at a distance of 61 feet to the south-west of the centre of the temple, and 364 feet to the north-east. The distance from the centre of Stonehenge to Salisbury Spire being 41,981 feet, the calculated corrections for parallax at the points of observation with reference to Salisbury Spire are:—

$$\begin{aligned}\text{Station } a &+ 4' 12'' \\ ,, \quad b &- 25 \quad 20.\end{aligned}$$

(1.) *Relative Azimuths*.—Theodolite at station *a*—

Salisbury Spire	0	0	0
N. side of opening in N.E. trilithon of the external ring	237	27	40
Tree in middle of clump on Sidbury Hill	237	40	20
Highest point of Friar's Heel	239	47	25
S. side of opening in N.E. trilithon	240	14	40
Middle ,, ,, ,, ...	238	51	10

(2.) *Absolute Azimuths*.—All the azimuths were referred to the Salisbury Spire, the azimuth of which was determined by observation of the Sun and Polaris.

(a.) *Observation of Sun, June 23, 1901, 3.30—3.40 P.M.*

Mean of observed altitudes of Sun	41°	26'	35"
Refraction	- 1'	4"	
Parallax	+ 6		
True altitude of Sun's centre	41	25	37

Latitude = 51° 10' 42". Sun's declination = 23° 26' 43".

Using the formula

$$\cos^2 \frac{1}{2} A = \frac{\sin \frac{1}{2} (\Delta + c - z) \sin \frac{1}{2} (\Delta + z - c)}{\sin c \cdot \sin z}$$

where *A* = azimuth from south, Δ = polar distance,
c = co-latitude, and *z* = zenith distance,

we get

Azimuth of Sun S. 75° 30' 30" W.
Mean circle reading on Sun 84 38 35

Azimuth of Salisbury Spire ...S. 9 8 5 E.

b. Observations of Polaris.—June 23, 1901. Time of greatest easterly elongation, calculated by formula $\cos h = \tan \phi \cot \delta$ is G.M.T. 1.34 A.M.

Azimuth at greatest easterly elongation, calculated by the formula

$$\sin A = \cos \delta \sec \phi,$$

is 181° 57' 0" from south.

Observed maximum reading of circle.....	256° 33' 0"
True azimuth of star	181 57 0
Meridian (S.) reading of circle	74 36 0
Circle reading on Salisbury Spire	65 28 0

Azimuth of Salisbury Spire S. 9 8 0 E.

The mean of the two determinations gives for the azimuth of Salisbury Spire S. 9° 8' 2" E. This result agrees well with the value of the azimuth communicated by the Ordnance Survey Office, namely, 9° 4' 8" from the centre of the circle, which being corrected by + 4' 12" for the position of station *a*, is increased to 9° 8' 20".

Hence from the point of observation *a* 9° 8' 20" has been adopted as the azimuth of Salisbury Spire.

We thus get the following absolute values of the principal azimuths from the point *a* :

Highest point of Friar's Heel	239° 47' 25"
	— 9 8 20
	230 39 5
or N.	50 39 5 E.
Middle of opening in N.E. trilithon.....	238 51 10
	— 9 8 20
	229 42 50
or N.	49 42 50 E.

The difference of 8½' between this and the assumed axis 49° 34' 18" is so slight, that considering the indirect method which has necessarily been employed in determining the axis of the temple from the position

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of the leaning stone, and the want of verticality, parallelism straightness of the inner surfaces of the opening in the N.E. trilithon we are justified in adopting the azimuth of the avenue as that of the temple.

Next, with regard to the determination of the azimuth of the avenue as indicated by the line of pegs to which reference is made in the body of the paper. The small angle between the nearest pegs A and B (which are supposed to be parallel to the axis of the avenue) was observed from station *a*, was measured, and the corresponding calculated correction was applied to the ascertained true bearing of the more distant peg B.

Thus

True bearing of peg B = ...	238°	35'	0"
Calculated correction to peg A = ...	0	12	8
True bearing of line AB	238	47	8
Bearing of Salisbury Spire	189	8	20

True bearing of a line parallel to the
axis of near part of avenue N. 49° 38' 48" E.

The mean of the three independent determinations by an observer was 49° 39' 6".

The calculated bearing of the more distant part of the axis of the avenue determined in the same manner by observations from station *a* is 49° 32' 54". The mean of the two, namely, 49° 35' 51", justifies the adoption of the value 49° 34' 18" as given by the Ordnance Survey for the straight line from Stonehenge to Sidbury Hill.

(3.) *Observation of Sunrise*.—On the morning of June 25, 1901, sunrise was observed from station *a*, and a setting made as near as possible on the middle of the visible segment as soon as could be done after the Sun appeared.

The telescope was then set on the highest point of the Friar's Hill and the latter was found to be 8' 40" south of the Sun.

Sun's declination at time of observation...	23°	25'	5"
Elevation of horizon at point of sunrise ...	0	35	48
Assuming 2' vertical of Sun to have been visible at observation, we have apparent altitude of Sun's upper limb.....	0	37	48
Refraction..... - 27' 27" }	- 0	27	18
Parallax + 0 9 }			
True altitude of upper limb.....	0	10	30
Sun's semi-diameter	0	15	46
True altitude of Sun's centre	- 0	5	16

From this it results that the true azimuth			
of the Sun at the time of observation = N. 50° 30' 54" E.			
And since azimuth of Friar's Heel	50	39	5
2' of sunrise should be N. of Friar's Heel	0	8	11
Observed difference of azimuth	0	8	40
Observed—calculated	0	0	29

The observation thus agrees with calculation, if we suppose about 2' of the Sun's limb to have been above the horizon when it was made, and therefore substantially confirms the azimuth above given of the Friar's Heel and generally the data adopted.

"The pear-shaped Figure of Equilibrium of a Rotating Mass of Liquid." By G. H. DARWIN, F.R.S., Plumian Professor and Fellow of Trinity College, Cambridge. Received October 21, 1901.

(Abstract.)

This is the sequel to a paper on "Ellipsoidal Harmonic Analysis," presented to the Royal Society in June, 1901.

Rigorous expressions for the harmonics of the third degree may be found by the methods of that paper, and the processes are carried out here. The functions of the second kind are also found, and are expressed in elliptic integrals.

So much of the results of M. Poincaré's celebrated memoir* on rotating liquid as relates to the immediate object in view is re-investigated, with a notation adapted for the use of the harmonics already determined. The general expressions for the coefficients of stability having been found, those for the seven coefficients corresponding to the harmonics of the third degree, as applicable to the Jacobian ellipsoids, are reduced to elliptic integrals.

The principal properties of these coefficients, as established by L. Poincaré, are enumerated. He has shown that the ellipsoid can bifurcate only into figures defined by zonal harmonics with reference to the longest axis of the Jacobian ellipsoid; that it must do so for all degrees; and that the first bifurcation occurs with the third zonal harmonic.

A numerical result given in the paper seems to indicate that as the ellipsoid lengthens, it becomes more stable as regards deformations of the third degree and of higher orders, and less stable as regards the lower orders of the same degree.

* 'Acta Math.,' vol. 7, 1885.

The numerical solution of the equation furnished by the vanishing of the coefficient corresponding to the third zonal harmonic shows that the critical Jacobian ellipsoid is such that its axes are proportional to 0.65066, 0.81498, 1.88583; and that the angular velocity ω and density ρ of the liquid are connected by the equation $\frac{\omega^2}{2\pi\rho} = 0.14200$.

This ellipsoid is the longest stable figure of Jacobi's series. A figure of the deformation of this critical ellipsoid by the third zonal harmonic is delineated in a plate. The so-called pear-shaped figure is seen to be longer than was indicated by M. Poincaré in his conjectural sketch.

Although this figure is almost certainly stable, absolute proof is still wanting. This proof can only be obtained by proceeding to a higher degree of approximation. An attempt is made to obtain this higher approximation, and the cause of failure and the difficulties of the problem are discussed.

“Sur la Stabilité de l'Équilibre des Figures Pyriformes affectées par une Masse Fluide en Rotation.” By H. POINCARÉ
Foreign Member R.S. Received October 29, 1901.

(Abstract.)

J'ai publié autrefois dans le Tome 7 des ‘Acta Mathematica’ un mémoire où j'étudie diverses figures d'équilibre nouvelles d'une masse fluide homogène en rotation. Presque toutes ces figures sont instables une d'elles cependant, qui est pyriforme, est très probablement stable. Mais la preuve directe de cette stabilité ne pourrait être obtenue que par de longs calculs. Le but du présent travail est de faciliter ces calculs, en donnant à la condition de stabilité une forme analytique aussi simple que possible. La question cependant reste indécise, parce que les formules analytiques n'ont pas été réduites en chiffres.

Il fallait d'abord obtenir une expression de l'énergie de gravitation d'une pareille figure en poussant l'approximation plus loin qu'on ne l'avait fait jusqu'ici. L'emploi des fonctions de Lamé peut conduire au résultat, mais on se trouve en présence d'une petite difficulté. Le potentiel d'un ellipsoïde, ou d'une couche ellipsoïdale, affecte de formes analytiques différentes selon que le point envisagé est à l'intérieur ou à l'extérieur de l'ellipsoïde. Il en résulte que dans chacune des intégrales il faudrait donner à la fonction sous le signe $\int$, tantôt une forme pour les parties de la surface pyriforme qui sont au dessous de la surface de l'ellipsoïde, tantôt une autre forme pour les parties qui sont au dessus. Mais j'ai reconnu que cette difficulté est purement artificielle et qu'on obtiendra encore un résultat final correct et

onnannt à ces fonctions sous le signe $\int$, soit *toujours* la première forme, soit *toujours* la seconde. En opérant de la sorte on commet une erreur sur *chacune* des intégrales, mais ces erreurs se compensent complètement dans la somme des intégrales.

Je me suis attaché ensuite à écrire l'inégalité qui exprime la condition de stabilité, et à réduire aux intégrales elliptiques les plus simples toutes les intégrales qui figurent dans cette inégalité.

November 21, 1901.

Sir WILLIAM HUGGINS, K.C.B., D.C.L., President, in the Chair.

Mr. F. W. Dyson, Lieutenant-Colonel David Bruce, R.A.M.C. (elected 1899), and Mr. A. Smith Woodward were admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

Major MacMahon, Dr. W. J. Russell, and Dr. A. D. Waller were by ballot elected Auditors of the Treasurer's accounts on the part of the Society.

The following Papers, received during the Recess, and published in full or in abstract, in accordance with the Standing Orders of Council, were read in title:—

"The Anatomy and Development of the Stem in the Pteridophyta and Gymnosperms." By EDWARD C. JEFFREY, Ph.D., Lecturer in the University of Toronto. Communicated by D. H. SCOTT, F.R.S.

"A Memoir on Integral Functions." By E. W. BARNES, M.A., Fellow of Trinity College, Cambridge. Communicated by Professor A. R. FORSYTH, Sc.D., F.R.S.

On Areal Induction." By GEORGE J. BURCH, M.A., F.R.S.

Further Observations on Nova Persei. No. 4." By Sir NORMAN LOCKYER, K.C.B., F.R.S.

An Attempt to ascertain the Date of the Original Construction of Stonehenge from its Orientation." By Sir NORMAN LOCKYER, K.C.B., F.R.S., and F. C. PENROSE, F.R.S.

150 Mr. G. H. F. Nuttall. *The New Biological Test*

"The pear-shaped Figure of Equilibrium of a Rotating Mass of Liquid." By G. H. DARWIN, F.R.S.

"Sur la Stabilité de l'Équilibre des Figures Pyriformes affectées par une Masse Fluide en Rotation." By H. POINCARÉ, For. Mem. R.S.

The following Papers were read :—

- I. "On Skin Currents. Part II.—Observations on Cats." By Dr. A. D. WALLER, F.R.S.
- II. "The New Biological Test for Blood in Relation to Zoological Classification." By Dr. G. H. F. NUTTALL. Communicated by Sir M. FOSTER, Sec. R.S.
- III. "Observations on the Cerebral Cortex of the Ape. Preliminary Communication." By A. S. F. GRÜNBAUM and Professor SHERRINGTON, F.R.S.
- IV. "On the Inheritance of the Mental Characters in Man." By Professor K. PEARSON, F.R.S.
- V. "On the Process of Hair turning White." By Professor ELLA METCHNIKOFF, For. Mem. R.S.

"The New Biological Test for Blood in Relation to Zoological Classification." By GEORGE H. F. NUTTALL, M.A., M.D., Ph.D. University Lecturer in Bacteriology and Preventive Medicine, Cambridge. Communicated by Sir M. FOSTER, K.C.E. Sec. R.S. Received November 2,—Read November 21, 1901

In recent publications which have appeared in the 'British Medical Journal'* and in the 'Journal of Hygiene'† I have described the technical methods whereby the so-called specific anti-sera may be produced, and in the article in the latter journal, the reader will find the literature on the subject exhaustively treated. The anti-sera are produced briefly as follows: Assuming that we wish to obtain an anti-serum for human blood, we inject human blood intraperitoneally into rabbits. After about five injections, given at intervals of three or more days, the rabbit is bled to death, and its blood serum collected. The serum of this rabbit will be found to have acquired the remarkable property of producing a precipitation immediately on its being added in small quantity to a dilution of

* 'Brit. Med. Journ.,' 1901 (11th May), vol. 1, p. 1141; (14th September) vol. 2, p. 669.

† 'Journ. of Hyg.,' 1901 (1st July), vol. 1, p. 387-387.

human blood-serum. If allowed to rest, the precipitated substance gravitates to the bottom of the tube. I have now tested upwards of 230 bloods obtained from animals of all classes of vertebrates with such anti-serum for human blood, and have, with the single exception of monkey bloods, obtained negative results throughout. Similarly, if rabbits are treated with the blood of the horse, dog, ox, sheep, &c., anti-sera are formed which produce precipitations only in the bloods of the animals whose blood was used for treatment, or, to a lesser extent, in the bloods of nearly allied animals.

The importance of the test from a medico-legal standpoint has been fully realised, and we can safely look forward to the test being put to practical use in the detection of crime. Whilst Uhlenhuth has proved that dried blood-stains can be used for the test, a solution of the dried blood being made for the purpose, I have shown that human blood which had putrefied for two months is capable of giving a reaction with its homologous anti-serum. I have, moreover, shown that human blood can be detected in a mixture of five or six different bloods brought into solution so that each blood is present but in the quantity of 1 : 500 or 1 : 600.

As stated above, the only bloods which gave a reaction similar to that of human bloods have been the bloods of different species of monkeys. Since my papers appeared I have had occasion to test eighteen kinds of monkey bloods. The reaction obtained with monkey blood only differs from that obtained with that of the human subject in degree. Monkey blood gives a feebler reaction than human blood with the anti-serum for human blood.

Accepting the classification of the Primates given by Flower and Lydekker,* we find that they are classified into two groups, the Lemuroidea (Lemurs) and Anthropeidea (Man and the Apes). As stated by the authors named, the view that the Lemurs belong to the Primates is largely traditional; they think they should, perhaps, be grouped in a distinct order. There are facts for and against this. Taking the Anthropeidea, we find them divided into the five families, Hapalidæ, Cebidæ (New World Apes), Cercopithecidæ, Simiidæ (Old World Apes), and Hominidæ (Man). According to Darwin, the Old World apes are more closely related in many respects to the Hominidæ than are the New World apes. And it is a striking fact, brought out by the tests I have made, that the New World monkeys give a less marked reaction with the anti-serum for human blood than do the Old World monkeys. On the other hand, the test gave a negative result when applied to the blood of two species of lemur (*L. xanthomystax*, *L. rufifrons*).

The eighteen monkey bloods tested were as follows: Hapalidæ (*Hapale pygmaea*, *Midas adipus*), Cebidæ (*Mycales seniculus*, *Uacaria*

* 'Mammals, Living and Extinct,' 1891.

rubicunda, *Cebus albifrons*), Cercopithecidae (*Macacus assamiensis*, *molgus*, *M. rhesus*, *M. ocreatus*, *Cercopithecus campbelli*, *C. patas*, *C. lalandi*, *C. melogenys*, *C. callitricha*, *Semnopithecus entellus*) (the chimpanzee, *Anthropopithecus troglodytes*, and Ourang-outa *satyrus*).

All of these bloods reacted to the anti-serum for human blood. New World monkey bloods less, and least of these the blood of the Hapalidae. I am at present attempting to estimate quantitative differences in the amount of reaction obtained.

When the experiment was tried with the anti-serum for dog blood the only bloods besides those of the domesticated dog were found to react were those of other Canidae (*C. aureus*, *C. procyonides*, *C. cerdo*). Similarly, the anti-serum for horse blood gave a reaction with the blood of the horse and donkey. The anti-sera for ox and sheep blood have given reactions, which indicate the existence of a "blood relationship" between certain of the ruminants. Whereas the anti-serum for ox blood acted powerfully on the blood of the ox and other members of the bovine section produced reactions, but to a lesser degree, with the bloods of certain species of the ovine section (Sheep and Goat), with the blood of several species of deer, the antelope and gnu. The anti-serum for sheep's blood gave almost as powerful a reaction with the blood of the closely related goat as it did with that of the different species of the bovine section and also produced lesser reactions with the bloods of the other ruminants above mentioned.

The above experiments, which are being prosecuted on a large scale, the attempt being made to obtain a variety of anti-sera, indicate a certain certainty that we possess in this test a most valuable aid in the classification of animals. I am at present engaged in preparing anti-sera for monkey blood, one of the objects being eminently practical. Just as in the case of the anti-serum for ox blood, which acted powerfully on ox blood and feebly on sheep's blood, and *vice versa*, by means of both the anti-serum for human blood and the anti-serum for monkey blood be able to differentiate the blood of man and monkey conclusively. This would scarcely have any practical application in this country, but it might very well be a matter of great medical importance in countries where there are monkeys. Thus, I recently received a letter from Mr. E. H. Hankin, of Agra, stating that he had come before him where it appeared essential to make a determination as to whether certain blood-stains were caused by human or monkey blood. In such cases it would be necessary to prepare anti-sera for the most prevalent genera or species of monkey.

The more powerful the anti-serum obtained the greater is its action upon the bloods of related species. For instance, the anti-serum for human blood produced no reaction with the blood of the

Hapalidæ, whereas a powerful anti-serum did produce a reaction, and proved what I may be permitted to call the "blood relationship" in the absence of a better expression.

Referring to the anti-serum for human blood, I may state that I have successfully produced it in rabbits by injections of old human pleuritic exudate preserved in a bottle with chloroform for five to six months. Similarly, some old anti-diphtheritic horse serum preserved for two years and seven months in the laboratory by means of trikresol also yielded an anti-serum for horse blood. The anti-sera produced in these cases was feebler than that which is produced by injections of fresh serum. Dilutions of these old preserved fluids gave the characteristic reactions with their homologous anti-sera. I have also found that the anti-sera may be preserved for months with chloroform, although there is no denying that they lose in power. Anti-serum which had been preserved for over seven months in sealed capillary tubes was likewise still effective, although less potent.

Through the kindness of Mr. Frank E. Beddard, F.R.S., Prosector of the Zoological Society's Gardens, and numerous friends, who are generously aiding me by sending blood-samples from various parts of the world, I am gradually gathering together considerable material for study. Wherever possible the fluid sera are being sent me preserved with chloroform. Dried sera, on the other hand, are sent on slips of pure filter-paper, upon which appropriate data are noted in pencil.

The results of the investigation indicate the necessity of not limiting the work to vertebrates alone, and many questions naturally suggest themselves, the solution of which may be attained by means of the biological test.

The assumption seems justified that we may, for instance, be able at some future date to determine chemical differences in the blood of the various races of man. We no longer need to rely solely upon morphological characters for the differentiation of species.

It is impossible to enter into details concerning the nature of the reaction here described; it is a subject for further study. Suffice it to say that it is exceedingly complex, but at the same time the most delicate of tests known.

"On the Inheritance of the Mental Characters in Man." By
KARL PEARSON, F.R.S., University College, London. Received
November, 2,—Read November 21, 1901.

(1.) Mr. Francis Galton, in his 'Natural Inheritance,' first, I believe, endeavoured to give a quantitative appreciation of the inheritance of the mental characters in man. Mr. Galton's data

were not very copious, and in default of a method of dealing quantitatively with characters not capable of exact scaling, it was not possible to deduce absolutely conclusive results. Still Mr. Galton brought good evidence to show that temper and artistic instinct were inherited characters. On November 19, 1899, a paper was read to the Royal Society showing how the inheritance of characters not capable of exact quantitative measurement might be deduced. In that paper I dealt with Mr. Galton's statistics, and showed that the fraternal correlation in the matter of temper was 0·3167, and the parental correlation in the matter of artistic instinct was 0·4039. These numbers are somewhat low and not altogether satisfactory. I purpose in this preliminary notice to give only a few results from some very elaborate observations which have been made in the course of the last few years.

(2.) The material was collected in two separate ways. In the first series—the Family Measurement Series—only physical characters were observed. This series was started six years ago, and upward of 1100 families, father, mother, and not more than two sons and two daughters, were measured. The series was closed two years ago, and last year Dr. Alice Lee completed the reduction of this very large mass of material. In its reduced form seventy-eight correlation tables have been formed, giving as many correlation coefficients bearing on direct or cross heredity. This is probably the most extensive series of inheritance coefficients—each based, as a rule, on upwards of 1000 pairs—which has yet been obtained.

My second series will be still more extensive; but it relates only to collateral—fraternal—heredity. It aims at observing a wide range of both physical and mental characters in pairs of school children. I have received most kindly aid from a great number of masters and mistresses in public schools, high schools, secondary and primary schools of all classes. This will be very fully acknowledged in the final publication of the results. But although the work has been in progress for three years, we have still only material enough to draw conclusions in the case of pairs of brothers, of whom more than 1000 cases have been observed.

The work has been carried on with the assistance from the Government Grant of a sum appropriated to this purpose in 1898. Without this aid it would not have been possible for me to purchase the necessary head-spanners or to circulate them among the schools.

(3.) Only three of the physical measurements of this extensive series have yet been reduced, and the sister-sister and sister-brother observations will have to be carried on for another year or two before they are sufficiently numerous. The whole material will then require two or three years for tabulation and calculation. But as the problem of the inheritance of the mental characters and their correlation

with the physical was occupying our attention in another field, the indefatigable Dr. Lee undertook the tabulation and calculation of the coefficients of heredity in the case of seven mental and three physical characters for pairs of brothers. The number of pairs dealt with in each case were 800 to 1000. The method adopted was that of the memoir on "The Inheritance of Characters not capable of Exact Quantitative Measurement."* Thus, under the heading *Conscientiousness* were two divisions, Keen and Dull, and the teacher might place a cross on either of these or on the dividing line. Similar divisions occurred in the other categories, except that *Intelligence* was given six and *Temper* three subdivisions, &c. The collecting schedules will be fully described when the whole bulk of material is finally reduced and published. My sole object in the present preliminary notice is to draw attention to the following results:—

Coefficients of Collateral Heredity.

Correlation of Pairs of Brothers.

<i>Physical Characters.</i> (Family Measurements.)		<i>Mental Characters.</i> (School Observations.)	
Stature	0·5107	Intelligence	0·4559
Forearm	0·4912	Vivacity	0·4702
Span	0·5494	Conscientiousness	0·5929
Eye-colour	0·5169	Popularity	0·5044
		Temper.....	0·5068
		Self-consciousness	0·5915
		Shyness	0·5281
(School Observations.)			
Cephalic index	0·4861		
Hair-colour	0·5452		
Health	0·5203		
Mean.....	0·5171	Mean.....	0·5214

The physical characters were measured or observed on two entirely different groups of individuals—in the one case, adults, in the other, children, were examined. Both groups, however, give very like mean results, *i.e.*, 0·5170 and 0·5172. Dealing with the means for physical and mental characters we are forced to the perfectly definite conclusion: *That the mental characters in man are inherited in precisely the same manner as the physical.* Our mental and moral nature is, quite as much as our physical nature, the outcome of hereditary factors.

The probable error of the coefficients given is about 0·02 at most; the differences between the individual values and their significance will be fully considered in the final memoir.

* 'Phil. Trans.,' A, vol. 195, pp. 79-150.

"On the Process of Hair Turning White." By E. METCHNIKOFF,
For. Mem. R.S. Received November 7,—Read November
21, 1901.

Although the fact of hair turning white is a most familiar one, its mechanism has not as yet been unveiled. The authors of works on hair and dermatology acknowledge their ignorance concerning this subject.

Having undertaken a study on atrophic processes, and especially on senile atrophy, my attention has been called to the atrophy of hair pigment so frequent in old people.

Observations on grey hair, or on hair beginning to turn grey, showed me that the atrophy of its pigment is due to the intervention of phagocytes of the hair.

These cells have a single nucleus and their very different aspect one from another is due to numerous amœboid prolongations of their protoplasm. They are derived from the medullary part of the hair and make their way out into its cortical layer, where they absorb the pigment granules, which they then remove from the hair.

If we consider hair, one part of which is already white and the other still pigmented, we find a great many of these phagocytes. They are supplied with greatly developed prolongations and become insinuated between the keratic cells of the peripheral layer.

In absolutely white hair the phagocytes filled with pigment become more and more scarce, and most frequently completely disappear.

It is thus indubitable that the phagocytes of the hairs swallow up the granular pigment of the cortical layer and transfer it elsewhere, the result being the complete whitening of such hair. On observing the root of hair beginning to whiten, we often find a great many phagocytes filled with pigment.

The whitening of the hair of old dogs proceeds by the same mechanism. We equally find here a great number of phagocytes supplied with numerous prolongations and stuffed with pigment granules.

The part played by phagocytes in the whitening of hair explains many phenomena observed long ago, but not as yet sufficiently understood. Thus, hair turning white in a single night, or in a few days, may be explained by the increased activity of hair phagocytes thus enabled to transfer the pigment in so short a time.

The mechanism of the whitening of hair through the agency of phagocytes, allows this case of atrophy to be classed under the general of atrophy of solid parts of the organism.

he Anomalous Dispersion of Sodium Vapour." By R. W. WOOD, Professor of Physics in the University of Wisconsin. Communicated by C. V. BOYS, F.R.S. Received June 10,—Read June 20, 1901.

[PLATES 1-3.]

Experimental proof of the dispersion formulæ of Sellmeier, Helmholtz, and Ketteler has for the most part been based upon observations made upon substances in the solid state or in the state of solution, where the range of absorption extends over a considerable portion of the spectrum, and the molecular condition is in all probability vastly more complicated than in the gaseous state.

In liquid oxygen and in the salts of erbium and didymium we have only examples, so far as I know, of substances other than gases which exhibit narrow absorption bands, and in these cases the absorption is scarcely strong enough to influence the dispersion to any marked degree. A careful study of the dispersion of some substance with absorption bands as narrow and strong as those of sodium vapour would be of great value in aiding us to pass judgment on the various modifications of Sellmeier's original theory.

The anomalous dispersion of sodium vapour in the immediate vicinity of the D lines was first observed by Kundt, and has since been studied by Becquerel and Julius, but none of these investigators traced the effect over a range of the spectrum greater than two or three times the distance between the lines.

In a previous paper* I have shown that a spectrum closely resembling the bright-line spectrum of incandescent sodium can be produced under certain conditions by the anomalous refraction of white light in a non-homogeneous atmosphere of sodium vapour, in precisely the manner in which Julius very ingeniously suggests that it may occur in the reversing layer of the sun, giving rise to the phenomenon of the "flash spectrum." In view of the seeming importance of the subject in its bearing on the subject of the dynamics of dispersion, and its possible connection with solar-physics, I have undertaken to determine whether sufficiently accurate quantitative data can be obtained, to make a test of the dispersion formula, as applied to sodium vapour, possible.

Though the absolute values of the refractive indices which have been found are not as satisfactory as is desirable, very interesting qualitative values have been obtained, and what is of especial interest, dispersion has been traced and measured throughout the entire range of the visible spectrum, the refractive index for all waves of

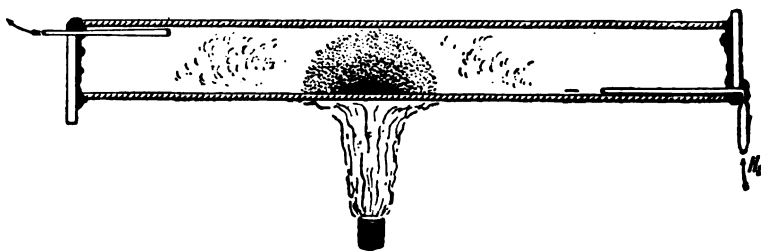
* *'Phil. Mag.'* May, 1901.

higher frequency than D_2 being less than one, and for all waves of lower frequency than D_1 , greater than one. In other words, I have been able to form a complete anomalous spectrum by means of sodium vapour, in which all of the colours between the extreme red and violet are present, with the exception of a range of perhaps twenty Ångström units at the D lines.

Previous investigations have been made exclusively with sodium flames, which have been made to assume a prismatic form. In view of the great uncertainty regarding the prism angle, density of the vapour, and its molecular condition, that is, whether dissociated or not, it seemed advisable to employ non-luminous vapour, obtained by heating metallic sodium in some inert gas or *in vacuo*.

The first experiments were made with a large prism of cast iron, furnished with windows of mica or thin plate glass, in which the metal was heated in an atmosphere of hydrogen. Very beautiful results were at once obtained, but certain peculiarities of the vapour's action showed that the refraction was due chiefly to the action of a non-homogeneous medium, the planes of constant density being horizontal. Great trouble was had with the windows, which soon became covered with a white deposit, which cut off most of the light. It being apparent, however, that the oblique faces played but a very small part, the effect being due almost wholly to the variable density of the vapour, it seemed best to make the most of this circumstance, and dispense with the trouble entirely by removing the glass plates to such a distance from the heated vapour that no deposit took place.

FIG. 1.



The arrangement finally adopted was simply a tube of glass about 30 cm. long, provided with plate-glass ends cemented on with sealing-wax. Hydrogen, dried by passage over calcium chloride, was conducted into and out of the tube by means of two fine glass tubes arranged as shown in fig. 1.

The diameter of the tubes should not be over 2 mm., and they should lie close against the sides of the large tube in order not to cut off any of the light. The most suitable diameter for the large tube is 2 cm. As the experiments to be described in this paper will, I feel

repay any one who takes the trouble to repeat them, and will be nely useful in illustrating the subject of anomalous dispersion to classes of advanced students, I shall describe in some detail the er of preparing and using these dispersion tubes which I have most advantageous. The ends of the tubes are first warmed hickly coated with sealing-wax; one of the glass straws is then l in position, and a small piece of plate glass, previously warmed, d against the wax, any crevices around the straw being closed wax. The leading-in tube is next placed in position, and a piece shly cut sodium (about 5 mm. on a side) inserted. The other w is then cemented to the tube, and the current of hydrogen d as soon as possible. Some experience is necessary properly to ste the hydrogen stream during the experiment. When the tube t heated much white smoke forms. If a stream corresponding to one bubble per second is allowed to flow, the smoke will usually up in a few minutes and give little trouble. The tube should be d by means of a Bunsen burner turned down low, the tip of the playing against the bottom of the tube. If a sodium flame is l behind the tube the formation of the vapour can be watched, appears almost jet black against the flame, though quite colour- n white light. The behaviour of the vapour is somewhat iar. It grows out from the sodium globule as a dark atmosphere a sharply defined surface, which clings to the globule with great ity. It resembles at first a thick growth of mould more than ing else that I can think of, and a sudden gust of hydrogen ly moves it at all. A wire pushed up through it drags a certain nt above the free surface in much the same manner as a stick d up through the surface of thick molasses would do. If the e inverted the black cloud clings to the upper surface, behaving e whole like a very viscous mass. It is even possible to dip some up on a wire.

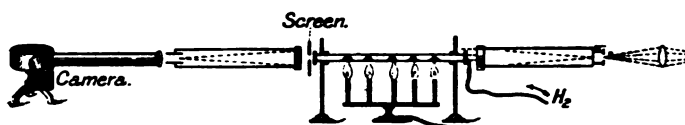
se peculiar physical properties of the metallic vapour I have as nly studied in a very superficial manner, and I mention them only because it appears to me that there is some connection en them and the optical behaviour of the medium.

n of the opinion that the apparent viscosity is an illusion, and the sharply-defined surface is merely the boundary at which condensation or chemical action (the hydrogen not being pure) ing place. The process of dipping the vapour up on the wire be explained by condensation on the wire followed by vapor- . A more careful study of the physical behaviour of the vapour e made some time in the future.

; apparatus employed in the study of the dispersion of the r was essentially identical with that used by Becquerel. The *of an arc lamp was focussed on the horizontal slit of a collimator,*

after traversing which the parallel rays passed lengthwise through the dispersion tube. A second lens brought them to a focus on the slit of a spectroscop, when the dispersion was to be studied by the method of crossed prisms, or in the focus of an eye-piece when the anomalous spectrum was to be viewed subjectively (fig. 2).

FIG. 2.



The first experiments were made by the method of crossed gratings, the spectrometer being furnished with a Rowland plane grating. This showed the sodium lines widely separated. It was at once apparent that far better results could be obtained with the diffraction grating than had ever been observed with prismatic flames. The branches of the diffraction spectrum on each side of the D line were perfectly sharp and steady, and the dispersion could be traced a considerable distance up and down the spectrum. On the slit of the spectrometer appeared, instead of the white image of the hole in the slit, a most beautiful anomalous spectrum, of great brilliancy and purity. The spectrometer was at once removed and an eye-piece placed in its place, when a most superb spectrum revealed itself. The appearance is shown in the coloured plate accompanying this report, though it is quite impossible to represent by means of pigments the sparkling brilliancy of the colours. (See Plate 1.)

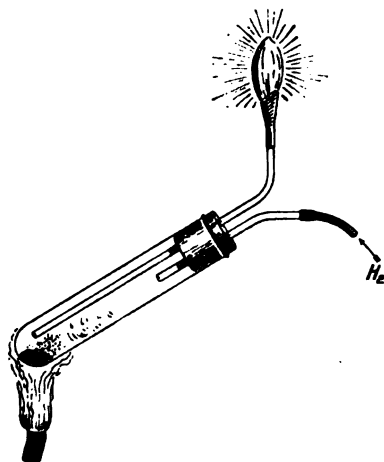
Before discussing this spectrum in detail it will be better to state the results of the experiments made by the method of crossed gratings. On first heating the tube the curvature of the spectrum between the D lines as well as on each side is observed, the appearance being identical with that figured by Becquerel, but in a few seconds the spectrum becomes so dense that total absorption of all the light between the D lines occurs. Julius expresses the opinion that this disappearance of light between the lines is only a result of the strong dispersion; that it is not absorbed but turned off to one side so that it does not enter the instrument. It appears to me, however, that this is not the case, for I have observed the same effect under conditions where the deflection seems quite out of the question. As I shall show, the breadth of the absorption band is sometimes twenty times the width of the spectrum comprised within the D lines.

The oppositely-curved branches adjacent to the region of absorption grow out rapidly as the tube grows hotter, the ends finally appearing out of the field of the instrument. A beautiful fluted spectrum appears in the red and the greenish-blue, which finally bleeds

egion in the blue almost entirely. Meanwhile the curvature of the spectrum increases in a most remarkable manner, and the entire red end is lifted high above the green-blue end. As the density of the vapour increases the red gradually fades away, leaving only the yellow and green and the remote blue and violet, the curvature increasing all the while. The fluted or channelled spectrum was described by Roscoe and Schuster about twenty-five years ago, but so far as I know no work has been done on it since. I have recently secured excellent photographs of it with a Rowland concave grating, from the extreme red to the violet, and find that it is much more extensive than has been supposed, for the flutings run right up to the absorption band at the D lines on *both* sides, though they are very faint on the side of shorter wave-lengths. This spectrum will be described in a subsequent paper.

Very satisfactory photographs of the dispersed grating spectrum were secured, some of which are reproduced. It was found impossible to maintain a sufficiently uniform density, at the low temperature, for a sufficient length of time to enable a negative to be secured showing the appearance before the light between the D lines vanished. I therefore went back to the old plan of using a prismatic flame. After some experimenting it was found that the most satisfactory flame was secured by passing hydrogen through a tube containing metallic sodium, strongly heated, and burning the gas at a flat jet piece made of platinum foil. An exceedingly dense and very uniform sodium flame is obtained in this way, which can be maintained almost indefinitely. The arrangement of the lamp is shown in fig. 3, the diagram requiring no description.

FIG. 3.



With this flame as a dispersing piece the photographs shown Plate 2, fig. 8, were obtained. In comparison with some of the other photographs this example is very poor, and I have included it merely to make the set complete. An exposure of about 10 minutes was necessary, with an "Erythro" plate. Fig. 9 was taken with one of the dispersion tubes, and shows what may be called the second stage. After the exposure was over, but before the plate was removed, a sodium flame was placed before the slit of the spectrometer, and sodium lines impressed on the plate in their proper position, to serve as a reference. For securing photographs of the dispersion result from still denser vapour more light was desirable, and a small prism was accordingly substituted for the grating. This prism bar resolved the sodium lines in the negatives, but gave excellent pictures of the spectrum under strong sodium dispersion. Figs. 10 and 11 were obtained in this way. In the latter the flutings in the red and green are discernible, though not very sharp.

As much of the fine detail is always lost in reproduction I have marked with arrows on fig. 11 the extreme points to which the curve branches adjacent to the absorption band can be traced in the original negative. Eye observations enable one to trace them much farther, but the tips are exceedingly faint, and the light is not very actinic.

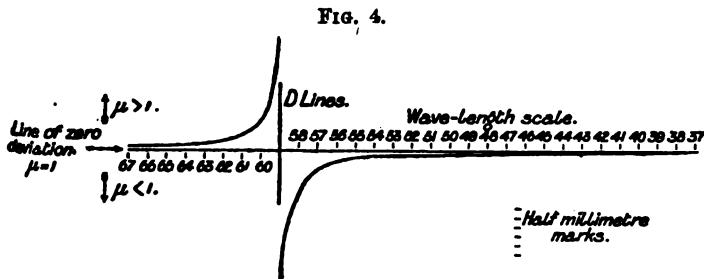
In order to secure accurate measurements of the dispersion in different parts of the spectrum I have made use of two different methods.

The photographs were not very suitable owing to the fact that during the exposure the density of the vapour varied somewhat, producing a slight blurring of the image. Measurements of the deviation of the continuous spectrum were accordingly made with a filar micrometer, an illuminated scale giving the wave-lengths at the points where readings were taken. Several minutes were necessary, however, to get anything like a satisfactory set of readings, and changes occurred even during that short period, as was at once apparent on repeated observations. I accordingly adopted the following method. The telescope of the spectrometer was removed and a telescope object of nearly two metres focus put in its place.

In the focal plane of this lens a plate of glass was firmly mounted on which a horizontal diamond scratch had been ruled. This diamond scratch was brought accurately into coincidence with the narrow continuous-spectrum image in its undeviated position. An eyepiece mounted behind the plate enabled the spectrum to be observed. When the dispersion tube was in good condition, and the deviated branches of the spectrum appeared sharp and steady, their positions on the plate were marked with a writing diamond, the line being drawn about the middle of the spectrum, which was only about a millimetre wide on the plate. The dispersion tube was then removed and various measurements were made to the arc, the bright lines being marked on the plate with the

umond. In this way a scale of wave-lengths was secured. For the termination of refractive indices we require, however, the angular viation of the different rays. This can be determined if we know the focal length of the lens forming the image of the spectrum on the slit of the spectrometer, and the actual deviations measured in millimetres the images of the first slit formed by light of various wave-length at this point. To secure a record of this a glass plate, ruled with half-millimetre lines, was placed over the slit of the spectrometer, and the position of the lines on the glass plate in the focus of the large lens was recorded with the writing diamond. This scale enabled the recorded deviations to be reduced to the actual deviations as they existed on the slit of the instrument.

The dispersion curve obtained in this manner, with the half-millimetre marks, is reproduced on the same scale as the original in fig. 4, the prismatic spectrum having been converted into a normal spectrum. It will be seen that for all wave-lengths shorter than those of the D lines, the refractive index is less than one, while for the rest of the spectrum it is greater than one. This is the only case that I know of in which we have a medium, beautifully transparent even in considerable thicknesses, in which light travels faster than in a vacuum.



To determine the refractive indices we require the angle of the medium prism, and here we encounter a grave difficulty, for it appears to be quite impossible to determine this directly. As I have said before, we are dealing with the optical equivalent of a prism, namely, a non-homogeneous medium, in which the vertical wave-front is retarded or accelerated in a progressively increasing amount as we pass from its upper to its lower edge. The equivalent prism appears on experiment to be bounded by concave instead of plane surfaces, in other words the effective angle is greater near the bottom of the prism than at the top. This can be shown by screening off different parts.

The lower part of the tube acting alone gives a much greater deviation than the upper. In practice I have found it best to screen

off the upper and lower portions, utilizing only the light which passes in the median plane, where the change in density appears to be quite uniform, this method having been used in preparing the dispersion curve alluded to above. The effective angle lies probably somewhere between 90° and 130° , but even this estimate is mere guess work.

If a single observation could be made with a vapour prism of known angle, in which we could be sure that the density was uniform, quantitative values could be assigned to determinations made with the dispersion tubes. I have accordingly tried in every way possible to obtain a prism fulfilling the required conditions. These endeavours have thus far met with only partial success, but an account of the devices tried may be of value to others working along similar lines.

Glass and mica are so quickly attacked by the sodium vapour that the use of these substances for prism faces seems to be out of the question. Added to this there is the difficulty of making gas-tight joints between plate-glass and metal which will stand a temperature but little below a red heat. Repeated failures to secure prisms in this way compelled me to seek some other method of giving the vapour the required form. I had observed that in glass tubes held before sodium flames the black vapour retreated before the approach of a cold obstacle. This suggested to me that it might be possible to do away with glass surfaces entirely, moulding the vapour into the required prismatic form by the proximity of cold bodies. Experiments along these lines were partially successful. Two pieces of thick walled iron tubing, the ends of which had been cut off at an angle were introduced into a glass tube, and the sodium placed in the clear space between the bevelled ends, as is shown in fig. 5. The ends of the

FIG. 5.



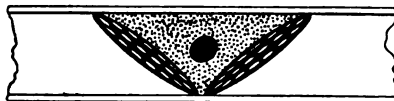
tube were closed in the usual manner and the whole mounted between the collimator and telescope, in such a position that the prism formed by the sloping ends of the iron tubes stood with its refracting edge vertical. This was necessary, for in any other position the refraction due to the non-homogeneity of the vapour would have made itself felt. With a vertical slit and a vertical prism no lateral deviation could result from this cause. It was hoped that the sodium vapour formed by sudden and rapid heating of the glass tube would refuse to enter the colder iron tube, and that a prismatic form would result from the bevelled ends. The slit of the spectroscope was illuminated with monochromatic light obtained by prismatic analysis, the wave-

length of which could be changed by turning the prism, which was furnished with a mirror in the manner described by Wadsworth.

On heating the tube the black vapour was seen to spread out and fill the prismatic clear space between the iron tubes, while the lateral deviation of the image of the slit, as observed in the telescope, indicated that, to a certain extent at least, the desired prismatic form had been secured. In the extreme red the deviation was very slight, but as the spectrum was advanced across the slit by slowly turning the prism the image in the telescope moved off to one side, the deviation in this direction reaching its maximum value just before the wave-length reached that of the sodium lines. At this point the image jumped abruptly to the other side just as we should expect it to do on crossing the D lines in the spectrum, and from now on the image slowly crawled back to its undeviated position. The focal length of the telescope was 460 mm., and the maximum deviation of the rays adjacent to the D lines on the red side, as measured by an eye-piece filar micrometer, was but 1 mm., while the deviation in the opposite direction of the rays on the other side of the D lines was 1.2 mm. The angle of the prism was 130, from which data we get the following values for the refractive index of the vapour for these two wave-lengths (relative to hydrogen) $\mu = 1.0005$ and $\mu = 0.9994$.

Similar results were obtained with the device shown in fig. 6, when two elliptical pieces of perforated sheet iron were used for moulding the vapour. The images formed in this case were blurred by diffraction.

FIG. 6.



I place very little value however on these figures, for I am of the opinion that the effective angle of the sodium prism is much less than the angle between the ends of the tubes, it seeming probable that the vapour bulges out into the tubes, especially near the centre. That this is to a certain extent the case is indicated by the fact that the image of the slit is not very sharp, though this may well be caused by the varying density of the vapour. I have not yet despaired of getting a prism bounded by plates of glass, about which there can be raised no question, though the problem is a difficult one, and observations will have to be made with great rapidity.

The deviations obtained by this method are very much less than those obtained with the dispersion tubes, indicating either that the equivalent angle is very large in the latter case (it may be nearly 180° for all we know) or that the angle of the prism formed by the iron tubes was less than the estimated value.

If we calculate the refractive indices from the data given by the curve traced with the diamond point, assuming the equivalent angle of the stratified vapour to be 130° , we get for the wave-lengths adjacent to the absorption band the values $\mu = 1.0024$ and $\mu = 0.9969$.

The value given by Becquerel (1.0009) is intermediate between the values obtained by the two methods, but I feel sure that his flame must have been non-homogeneous, and his numerical results more or less inaccurate in consequence.

For the exhibition of the actual spectrum produced by a prism of sodium vapour a long dispersion tube with a battery of four of five prisms gives the best results. A small Bunsen burner should be used for each of the fragments of sodium, which should be at least 6 or 8 cm. apart. (This arrangement is shown in fig. 2, p. 160.)

The coloured drawing of the spectrum (Plate 1), to which I have already alluded, was made when the spectrum was obtained in this manner. A single prism gives a very pretty anomalous spectrum, but the magnificent effect produced by the battery makes the slight amount of extra trouble well worth while. If the electric arc is employed as the source of light extreme violet will be found to occupy the position of the undeviated image of the slit. Then comes the blue, sometimes in contact with the violet and sometimes slightly separated by a fine dark line, owing to the fact that the violet light comes from the fluted carbon band of the arc, which is separated from the blue by a comparatively dark region. Then comes a wide gap corresponding to light absorbed by the sodium vapour in the blue-green region (the channelled spectrum), and above this a beautiful flare of colour ranging from blue-green through grass-green to yellow. The red and orange portion of the spectrum is on the other side of or below the undeviated image, forming another brilliant flare of colour. It is separated from the violet by a wide dark band, due to the absorption in the vicinity of the D lines. If the density of the vapour is increased by heating the tube to a higher temperature the red flare extends lower down, grows fainter, and finally fades away owing to the presence of the fluted absorption bands in the red. The green and blue persist, however, becoming more widely separated, but finally the green disappears almost entirely. It is best to arrange the gas cock so that the height of the flames can be controlled without leaving the eye-piece, for it is surprising how slight a change is necessary to completely alter the general appearance of the spectrum.

The glass tube should not be allowed to cool until the experiment is at an end, otherwise it will immediately fly to pieces as soon as flame is again applied to it.

While glass answers very well if the experiment is to be of short duration, sheet iron is much more satisfactory. Suitable tubes can be made by any tinsmith. They should be made of thin sheet iron,

the turned-over seam hammered until a tight joint is formed. These tubes can be heated and cooled any number of times, and can be kept in operation for an hour or two, at the end of which time the sodium is generally used up, a moss-like deposit of oxide gradually filling up the tube. These tubes can be used over and over again without deterioration, and are most satisfactory in every respect. Their only fault lies in their conductivity, the sealing-wax softening and the glass plates falling off, but this can be prevented by wrapping a strip of cloth around each end, and wetting it from time to time. I have had one tube made with water-jackets at each end, but it seems to have no special advantage, and is more complicated. Porcelain tubes are quite satisfactory, but I prefer the iron on the whole.

By employing a tube of about 5 cm. diameter I have succeeded in projecting the anomalous spectrum, but the appearance is so very inferior to that of the phenomenon when seen subjectively that I should never think of employing the method.

No trace of any influence of the fluted absorption bands on the dispersion has been detected.

It is a matter of some interest to know what becomes of the energy absorbed by a sodium flame, or by non-luminous sodium vapour. Most of it is undoubtedly spent in raising the temperature of the vapour; this rise of temperature I have shown experimentally by making the dispersion tube the chamber of a radiophone, and illuminating it with intermittent light. It was found that the radiophone sang loudly when illuminated by an intense beam of light which contained waves of the frequency of the sodium radiations, but became almost silent when these frequencies were removed.

Prismatic analysis was found to weaken the light too much, consequently a very thin film of cyanine was used which was quite opaque to the region of the D-lines while transparent to the rest of the spectrum. Sunlight concentrated by a large mirror was employed for the illumination.

It has always appeared to me thinkable that the absorbing sodium ions, when in resonant vibration, might emit a certain amount of light laterally. Kayser mentions in his new work on spectroscopy an experiment made by Miller, in an attempt to detect this phenomenon if it existed. The possibility of such a lateral radiation is discussed by Drude in his '*Lehrbuch der Optik*' (page 493), and is shown to depend on a small value of the constant r in the equation of motion of an ion vibrating under the influence of a periodic electric force. For sodium vapour r is calculated to be something between 10 and 100. If it were equal to one we should expect a lateral radiation of light, which, says Drude, has never been observed.

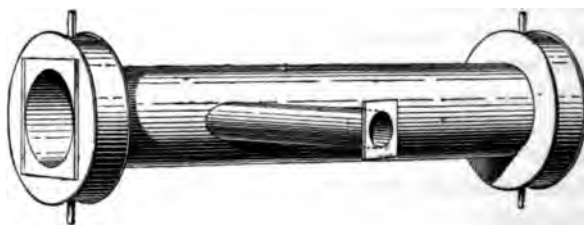
While experimenting with the very intense beam of sunlight in the

radiophone experiment I observed what appeared to be a lateral radiation of a clear green light by the vapour.

My first thought was that it was light scattered by the small particles of oxide which are always floating about. If this were case the spectrum of the light should be that of white light after transmission through sodium vapour. An examination with the spectro-scope, however, showed that this was not the case, for the spectrum consisted of a diffused red band, a very narrow bright band in the case where the D lines occur, and a diffused band in the green, in which flutings seemed to be present. The general appearance of the light reminds one of the fluorescence of uranium glass.

I am at the present time engaged in an investigation of the spectrum of this fluorescent light, but am not yet prepared to say whether the flutings in the green are discontinuities in the emission or due to the absorption of the non-luminous vapour surrounding the fluorescent vapour. A careful photographic study will be necessary before this can be decided, for which investigation I am employing an iron tube provided with copper jackets at each end, through which water circulates. (Fig. 7.) The light emitted laterally is observed through a second iron tube inserted at a right angle, and provided with a glass window. The sodium is placed at the junction of the tubes, which is then raised to a red heat with a blast lamp.

FIG. 7.



So far as I am able to judge by passing the light causing the fluorescence through colour screens, the wave-length of the emitted light corresponds very nearly to that of the exciting vibrations. Interposing a thin cyanine screen, which removes the light in the neighbourhood of the D lines, causes no apparent diminution in the intensity of the emitted light; but the spectroscope shows that the bright band at wave-length 5890 has disappeared. A film stained with aurantia, which removes everything below the "b" group, greatly diminishes the intensity of the fluorescent light, and changes its colour to red. A solution of cuprammonium of moderate strength removes from the light all of the wave-lengths which are capable of exciting the fluorescence. A more careful investigation will be neces-

sary before deciding whether the wave-length of the emitted light is greater than that of the exciting vibrations. My impression is that the bright narrow band in the yellow is slightly on the red side of the D lines, but I do not feel absolutely sure of it. It will be interesting to see whether the fluorescence persists for an appreciable time after the light is cut off, which can be easily determined with an especially designed phosphoroscope, a matter which I expect to take up next.

A quantitative study of the absorption of the vapour is extremely desirable, though the experimental difficulties will be very great. As I have already said, the width of the band at the D lines is often from ten to fifteen times the distance between the lines. Julius has, however, called attention to the fact that we must be on our guard against attributing the absence of light in the spectrum to absorption, when the conditions are such that the rays may have been merely turned to one side by refraction. This he believes, if I read his paper rightly, is the true explanation of the widening of the D lines in the absorption spectrum. To eliminate the possibility of this lateral deviation it is necessary, if we are working with a non-homogeneous medium, to arrange matters so that the rays of light are perpendicular to the surfaces of equal refractive index, instead of parallel, as is the case in the dispersion tubes. One obvious method of getting around the difficulty would be to vaporise the sodium in a vacuum, thus doing away with the variations in the density, but this necessitates contact between the corrosive vapour and the glass. A better plan appeared to be to send the light through the stratified vapour in such a direction that no lateral deviation could result, a matter of some difficulty until the expedient occurred to me of using the surface of the molten metal as a reflector, the rays thus twice traversing the non-homogeneous medium in a direction perpendicular to the equi-indicial surfaces.

A photograph of the absorption spectrum of the vapour in the red and yellow region, obtained with a 10-foot concave grating, is reproduced on Plate 3, fig. 12. The D lines were photographed just below the spectrum for comparison. The flutings in the red are well shown in this picture, which is but one of a large number that have been taken. Measurements of wave-length have not yet been made, as I feel confident of getting better plates in the near future. The fine dark lines can be found on both sides of the heavy band at the D lines, and I have found that they are present throughout the entire spectrum. An enlargement of a portion of fig. 12 is shown in fig. 13, which gives a better idea of the appearance of the fluted bands.

On increasing the density of the vapour the red end of the spectrum finally disappears, and by employing a very powerful light in combination with the method above alluded to of reflecting the light from the

mirror of molten sodium, I find that the spectrum consists of two exceedingly brilliant narrow green bands with a narrow dark line between them, and a band in the violet which is partially filled with flutings. This spectrum I have observed but once, and have given it no careful study as yet.

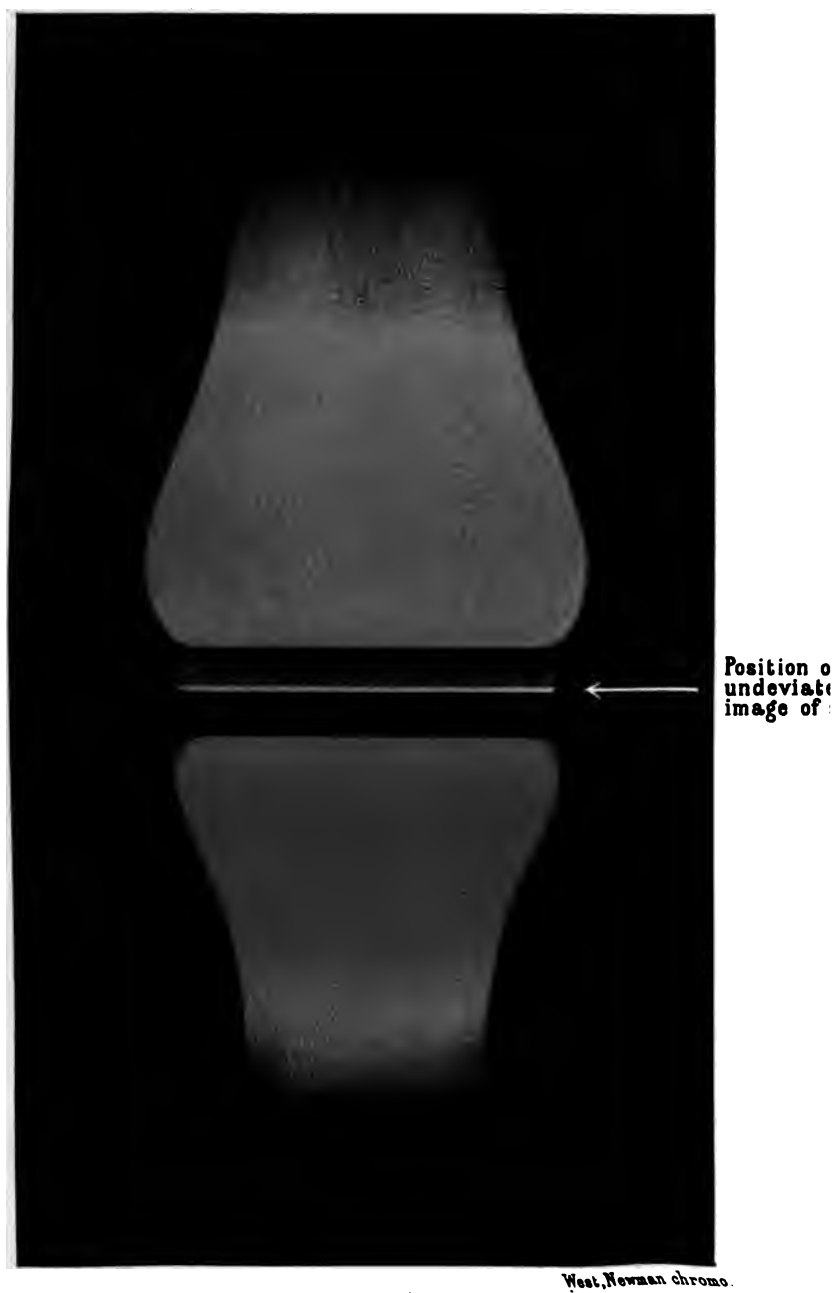
The flutings in the green-blue portion of the spectrum are similar in appearance to those in the red, but they make their appearance when the vapour is less dense, this part of the spectrum being blotted out entirely when the red bands are at their best.

I have not yet investigated the question of whether an increase in the thickness of the layer of vapour is the equivalent of increasing its density, but I feel quite sure that such is not the case. There remains too the question of pressure. All of the experiments that I have made thus far have been at atmospheric pressure, the variations in density being in all probability merely dilution of the metallic vapour with hydrogen.

Sodium vapour possesses another property which requires further investigation, namely, the power of emitting light in virtue of temperature alone. This was first observed by Evershed.* Iodine and a number of other substances behave in a similar manner, the temperature necessary to produce the luminous radiations being but little above a red heat. On repeating Evershed's experiment with iodine, my first impression was that anomalous dispersion might be at the bottom of it. The vapour was heated in a tube, and the purple luminosity or glow hung close to the inner wall, which was red hot. It seemed quite possible that the phenomenon might be a mirage effect, the rays of suitable wave-length coming from the red-hot wall moving in curved paths through the non-homogeneous vapour. To test this I devised a method by which this possibility was entirely eliminated, and found that the glow was more brilliant than ever. A spiral of platinum wire mounted in a small flask in which iodine was vaporised was raised to a bright red heat by an electric current. The convection current of iodine which rose through the spiral was luminous to a height of about 2 cm. above the spiral, waving from side to side in the form of a reddish-purple flame. Focussing the image of an electric arc on the spiral increased its luminosity ten-fold, without, however, altering the intensity of the glowing vapour. Probably the luminescence of sodium could be shown in the same way, though the one experiment that I tried with it gave negative results.

I expect in the near future to investigate all of the questions which I have touched upon in the latter part of this paper, for it appears to me that, with proper coaxing, sodium vapour may furnish us with much information regarding molecular dynamics. The results obtained up to the present time are only a small beginning, but appear to indi-

* 'Phil. Mag.,' vol. 39, 1895.



Anomalous Spectrum produced by Prism of Sodium Vapour..



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FIG. 8.



FIG. 10.



FIG. 9.



FIG. 11.

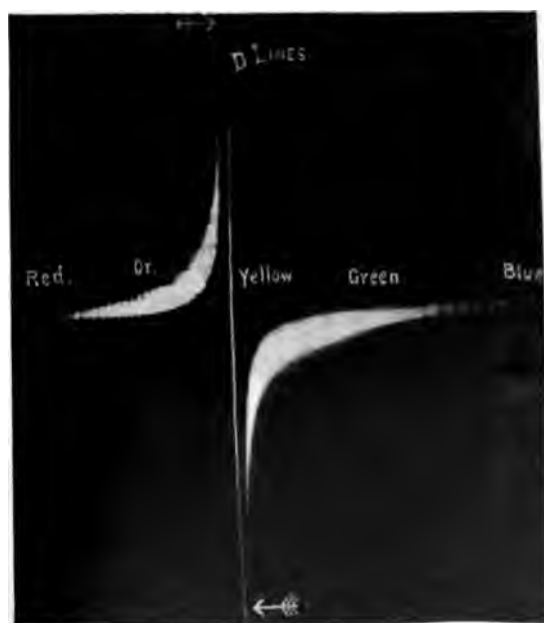
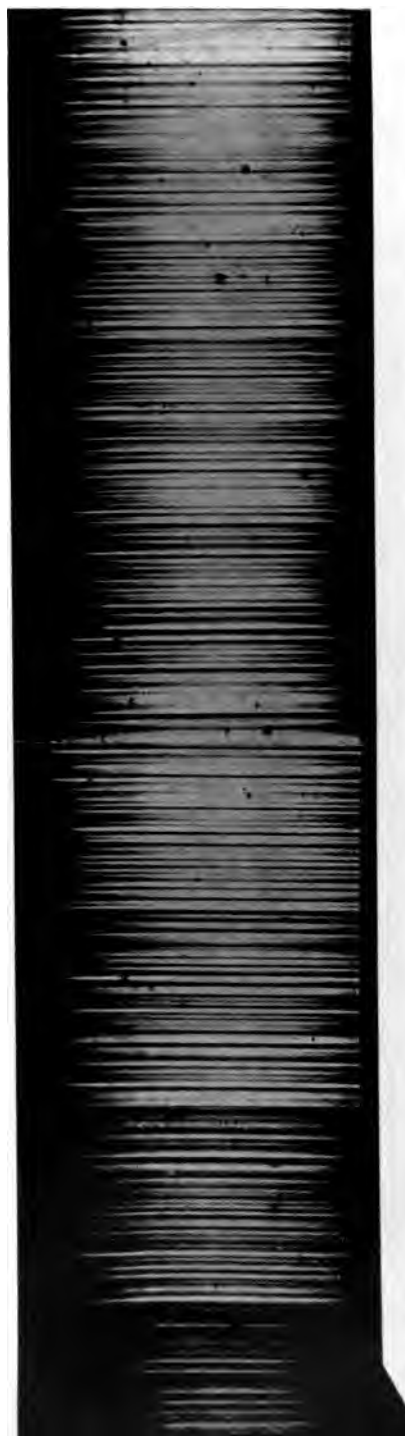






Fig. 13.





cate that in sodium vapour we have a medium which approaches more nearly to the ideal absorbing medium, with but a single natural period of vibration, than any substance heretofore investigated.

Of course I am speaking here only with reference to the natural vibration which appears to influence the dispersion. Strictly speaking, there are two natural periods, of course, which influence the velocity of the light in the medium, but when the medium is very dense the condition certainly approaches very nearly to that of a single period medium. As I have said before, the fluted absorption bands are without influence on the dispersion, at least their influence is too slight to be detected by the methods that have been employed thus far.

“On Skin Currents. Part II.—Observations on Cats.” By
AUGUSTUS D. WALLER, M.D., F.R.S. Received October 7,—
Read November 21, 1901.

In the first part of the present investigation* I have stated, as one of the principal conclusions with regard to the frog's skin, that the normal electrical response of the excited skin is of outgoing direction.

The chief object of the following observations was to ascertain whether or no similar effects of outgoing direction are manifested by the pad of the cat's foot, this having been, since the first observations of Hermann and of Luchsinger, the chosen object upon which to demonstrate the cutaneous (epithelial and glandular) currents aroused by nerve-stimulation.

I have examined the cutaneous currents, A indirectly aroused by nerve-stimulation, B directly aroused by electrical excitation of the skin itself in the manner described and figured in my previous communication (*loc. cit.*, p. 481).

A. INDIRECT EXCITATION.—The animals were decapitated, and used in the first instance for the observation of indirect effects and their possible modification in consequence of arrested circulation. In every case, without exception, the electrical effect of excitation of the sciatic nerve upon the pads proved to be an ingoing current, as described by Luchsinger and by Hermann.† The effect gradually declined with lapse of time, and disappeared within 1 hour after decapitation, without exhibiting any change of sign or other modification.

Although it was not my purpose to pay particular attention to this point, I may take the opportunity of stating that the experiment, as

* “On Skin Currents. Part I.—The Frog's Skin,” ‘Roy. Soc. Proc.,’ June 6, 1901, vol. 68, p. 490.

† Kendall and Luchsinger, “Zur Theorie der Secretionen,” ‘Pflüger's Archiv,’ vol. 13, 1876, p. 212; Hermann and Luchsinger, “Ueber die Secretionsströme der Haut bei der Katze,” ‘Pflüger's Archiv,’ vol. 17, 1878, p. 310.

made upon a freshly killed animal, affords a very convenient means of class-demonstration of the fundamental fact. The response is elicited by stimulation of the sciatic nerve after the latter has ceased to provoke muscular contraction, and can then be most conveniently demonstrated (and recorded) without resort to the curarisation and artificial respiration necessary in the case of the living animal. The method and its results will be made sufficiently apparent by the following experiments and figures:—

Experiment 1.—Electrical Response of the Skin to Indirect Excitation through the Sciatic Nerve.

Cat decapitated—Both sciatics exposed and cut—Large pad of each hind foot led off to galvanometer by unpolarisable electrodes—Tetanic excitation of peripheral end of one or other sciatic for 5 seconds at intervals of 5 minutes—Berne coil at 1000 units supplied by two Leclanchés.

Deflection + signifies that the skin of the right side is electro-positive, i.e., that it is traversed by an ingoing current (directed from left side to right through the galvanometer).

Time after decapitation. Mins.	Deflection by excitation of left sciatic. Volt.	Deflection by excitation of right sciatic.	Remarks.
15	-0·0100	—	Muscles contract.
20	-0·0150	—	Do.
25	-0·0110	—	Do.
30	—	+0·0050	Do. faintly.
35 and 36	-0·0100	+0·0160	No contraction.
40 and 41	-0·0060	+0·0100	Do.
45 and 46	-0·0010	+0·0060	Do.
50 and 51	Nil	+0·0010	Do.
55 and 56	Nil	Nil	Do.

The effect of excitation was thus in each instance an ingoing current of the skin of the excited side.

Experiment 2.—Similar Experiment. Excitation of Right Sciatic at Intervals of 10 minutes.

Time. mins.	Deflection.	Remarks.
20	+0·0050	—
30	+0·0045	Faint contraction.
40	+0·0035	No contraction.
50	+0·0020	Do.
60	+0·0005	Do.
70	Nil	Do.

Photographs of the effects 30 and 50 minutes after decapitation are given below. The first is taken on a slowly moving plate with tetanisation of the sciatic lasting for 30 seconds. The second is taken on a more rapidly moving plate with tetanisation lasting for 5 seconds, as shown by the signal line. The lost time in this (and in other) instances = 3 seconds. The effects are "ingoing."

FIG. 1. (4217.)

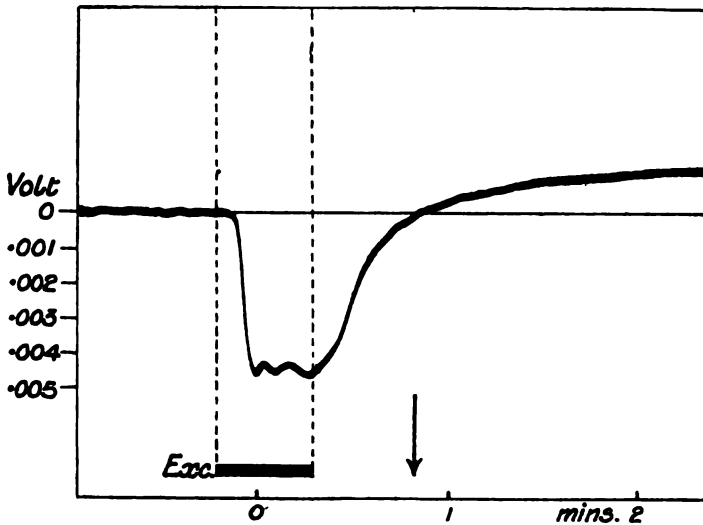
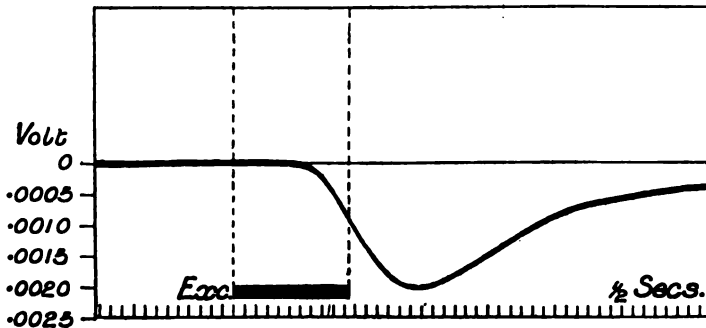


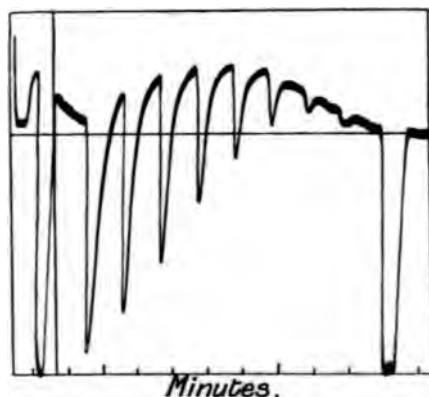
FIG. 2. (4218.)



Experiment 3.—Similar Experiment. Excitation of Right Sciatic at Intervals of only 1 minute. Rapid Exhaustion of the (ingoing) Response. No marked Alteration of Resistance. ? Increase by Drying.

Time after decapitation. Mins.	Voltage of response.	Resistance of pad (corrected for R. of electrodes).
25	—	100,000 ohms.
26	0·0050	
27	0·0085	
28	>0·0100	
29	—	100,000 „
30	Galvanometer shunted, photograph begins.	
31	—	100,000 ohms.
32	0·0100	
33	0·0093	
34	0·0080	
35	0·0060	
36	0·0040	
37	0·0020	
38	0·0005	
39	trace	
40	—	110,000 „

FIG. 3. (4220.)



B. DIRECT EXCITATION.—A pad of the cat's foot, cut off 1 to 48 hours after death, and set up between unpolarisable electrodes as previously described (*loc. cit.*, p. 481), generally manifests a small and gradually increasing ingoing normal current, not exceeding, as a rule, 0·0100 volt. With lapse of time this current gradually falls again. It is noteworthy that its direction (ingoing) is from the intact external surface towards the injured cut surface.

If after exact compensation of this normal current a single induction shock is sent through the pad, either in the ingoing or in the outgoing direction, the effect—or, properly speaking, the after-effect—is in nearly every instance outgoing, as in the case of the frog's skin. The few instances in which I have witnessed an ingoing response have been in the case of fresh skin, taken soon after decapitation, and tested by comparatively weak induction shocks. (I find in my laboratory notes only one instance of a late ingoing response—3 days after decapitation.) In such cases the ingoing response has been of comparatively low voltage, and has given place to an outgoing response at the end of a few hours. From review of a considerable number of observations I conclude that outgoing response is the rule, ingoing response the exception. I have kept the former under systematic observation up to 60 hours *post-mortem*, but have found under the conditions of observation that it may disappear at an earlier period from the skin of ill-nourished animals.

The physiological action of the skin is indicated: 1, by its invariable direction with both directions of excitation; 2, by its complete abolition after immersion of the skin in hot water.

A series of responses provoked at short intervals exhibits summation. Tetanisation by alternating induction shocks in both pairs of directions, excites a larger response than that aroused by a single induction shock.

These several points will be made sufficiently clear by the following experiments and figures:—

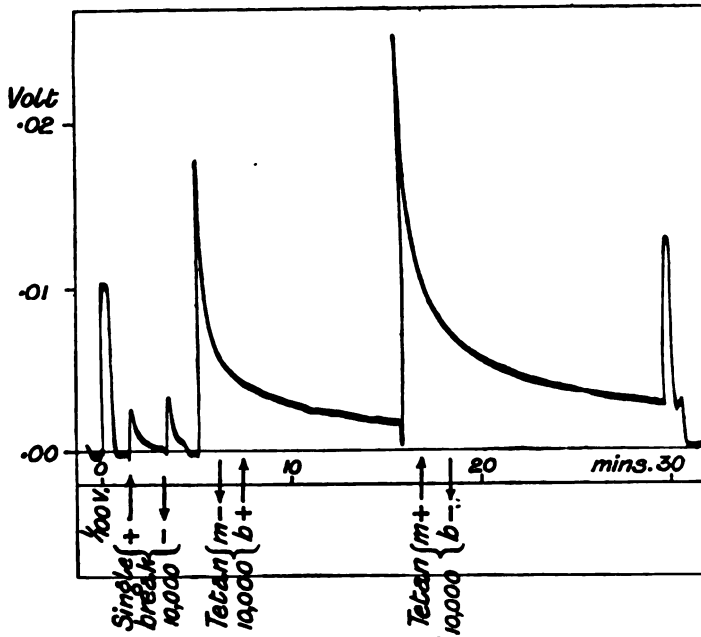
Experiment 4.—Direct Excitation. Same Cat as that of Experiment 2. Large pad cut off and set up between unpolar electrodes 9 hours after decapitation. Compensation. Direct Excitation in + (outgoing) and - (ingoing) directions. Berne Coil. 8 Leclanchés in primary circuit. 10,000 units.

Time.	Excitation.	Response.	After immersion in hot water.
9 hours	Tetan. $\left\{ \begin{array}{l} m. - \\ br. + \end{array} \right\}$	+ 0·0056 volt.	
	Do. $\left\{ \begin{array}{l} m. + \\ br. - \end{array} \right\}$	+ 0·0100 "	
24 hours	Single break +	+ 0·0025 "	- trace.
	-	+ 0·0032 "	+ trace.
	Tetan. $\left\{ \begin{array}{l} m. - \\ br. + \end{array} \right\}$	+ 0·0175 "	+ 0·0004
	Do. $\left\{ \begin{array}{l} m. + \\ br. - \end{array} \right\}$	+ 0·0242 "	- 0·0004

The response to both directions of excitation is outgoing; it is greater to tetanising currents than to single shocks; it is greater the day after death than on the day of death; it is abolished by immersion in hot water, the small residual deflections observed being such as would be caused by ordinary polarisation.

The 24-hour responses were recorded as under; they were throughout of outgoing direction, by both directions of single break currents, and by both pairs of directions of tetanisation.

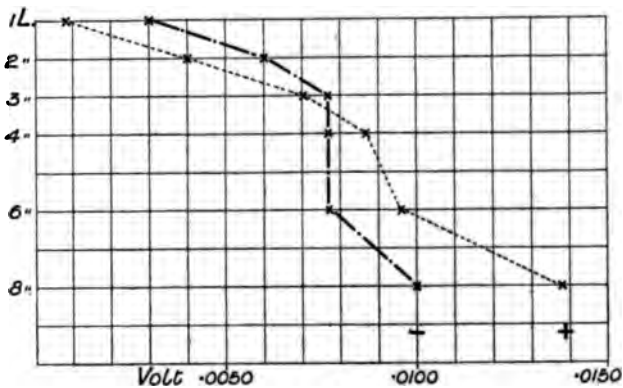
FIG. 4. (4219.)



Experiment 5.—Direct Excitation. Cat's Pad put up 8 hours after decapitation. Tetanisation for 5-sec. periods, by Berne Coil at 10,000 units with 1·2 8 Leclanché cells in primary circuit. Readings taken in scales of increasing and diminishing strengths of Excitation. (+ = out-going.)

In primary circuit.	Direction of break.	Double series of readings.	Mean.	Mean voltage.
1 Leclanché	+	+10 — 2	+ 4	0·0008
1 „	—	+15 +15	+15	0·0030
2 „	+	+30 +10	+20	0·0040
2 „	—	+35 +25	+30	0·0060
3 „	+	+45 +25	+35	0·0070
3 „	—	+45 +33	+38	0·0076
4 „	+	+52 +36	+44	0·0088
4 „	—	+42 +34	+38	0·0076
6 „	+	+60 +36	+48	0·0096
6 „	—	+52 +24	+38	0·0076
8 „	+	+74 +64	+69	0·0138
8 „	—	+54 +50	+52	0·0104

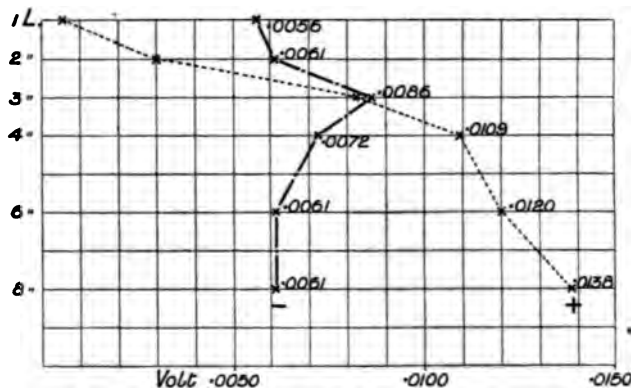
FIG. 5.



Experiment 6.—Direct Excitation. Fresh Pad of same Cat on the 3rd day after death. Excitation as in Experiment 5. Outgoing response throughout. Considerable Diminution of Resistance during Experiment.

In primary circuit.	Direction of break.	Readings on scale.	Mean readings.	Mean voltage.	Conductivity.
1 L.	+	- 2 + 3	+ 1	+ 0·0004	21
	-	+ 12 + 12	+ 12	+ 0·0057	
2 L.	+	+ 7 + 9	+ 8	+ 0·0030	26
	-	+ 18 + 15	+ 16	+ 0·0061	
3 L.	+	+ 30 + 18	+ 24	+ 0·0083	29
	-	+ 28 + 22	+ 25	+ 0·0086	
4 L.	+	+ 42 + 30	+ 36	+ 0·0109	33
	-	+ 25 + 23	+ 24	+ 0·0072	
6 L.	+	+ 45 + 50	+ 47	+ 0·0120	39
	-	+ 23 + 25	+ 24	+ 0·0061	
8 L.	+	+ 40 + 60	+ 50	+ 0·0138	36
	-	+ 23 + 22	+ 22	+ 0·00610	

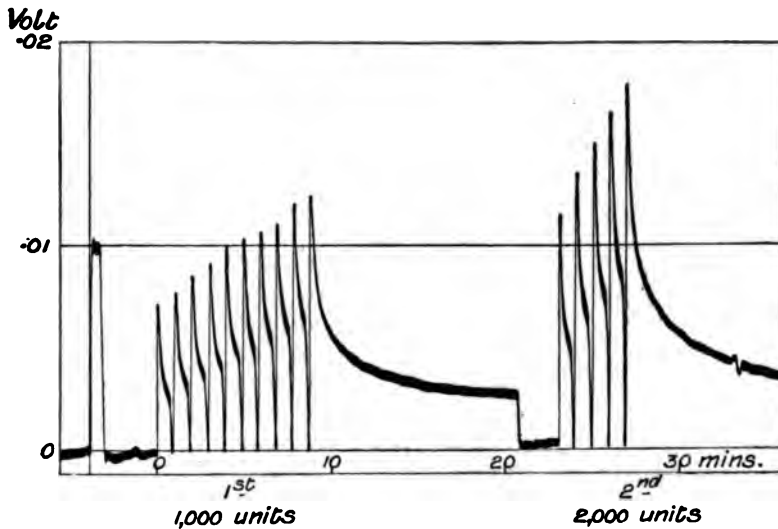
FIG. 6.



Experiment 7. Direct Excitation.—To illustrate summation of effects. Large pad of cat's foot set up 24 hours *post mortem*. Tetanisation for 5 seconds—at intervals of 1 minute. Break shock ingoing; response outgoing. Berne coil; 2 Leclanchés, at 1000 units in 1st group, at 2000 units in 2nd group. Compensation was adjusted at the commencement of each group, and left unaltered during the series; the galvanometer being short-circuited during each tetanisation, the spot then falls to the zero level.

	1st	5th	10th response.
1st series (1000)	+0·0070	+0·0100	+0·0125 volt.
2nd series (2000)	+0·0114	+0·0178	— „

FIG. 7. (4182.)



Experiment 8.—Direct Excitation. Response at first ingoing (—), subsequently outgoing (+). Berne Coil; 2 L. in primary circuit.

Time after decapitation.	Single-break currents.		Tetanising currents.		Tetanising currents.	
	10000 +	10000 —	1000 +	1000 —	10000 +	10000 —
½ hour	—0·0012	—0·0010	—0·0020	—0·0010	—0·0018	—0·0020
6 hours ...	+0·0005	+0·0010	+0·0006	+0·0006	+0·0092	+0·0068
24 hours ...	—	—	+ trace	+ trace	+0·0110	+0·0062

Experiment 9.—Direct Excitation. Similar Observation, more closely taken, with Photographic Records. Berne Coil ; 2 L. ; Tetanisation ; + and - signify outgoing and ingoing.

Time after decapitation.	Excitation (+ and - denote) direction of break.	Response.
40 min.	1000 +	- 0·0020
	1000 -	- 0·0013
	10000 +	- 0·0019
	10000 -	- 0·0010
90 min. (same pad)	1000 +	+ trace.
	1000 -	+ trace.
	10000 +	+ 0·0020
	10000 -	+ 0·0020
135 min. (fresh pad)	1000 +	- 0·0020
	1000 -	- 0·0010
	10000 +	- 0·0010
	10000 -	+ 0·0004, - 0·0005
	10000 +	+ 0·0003, - 0·0004
	10000 -	+ 0·0008, - ?
4½ hours (same pad)	10000 +	+ 0·0010
	10000 +	+ 0·0016
	10000 -	+ 0·0022
24 hours (same pad)	1000 +	Nil.
	1000 -	Nil.
	10000 +	+ 0·0017
	10000 -	+ 0·0013

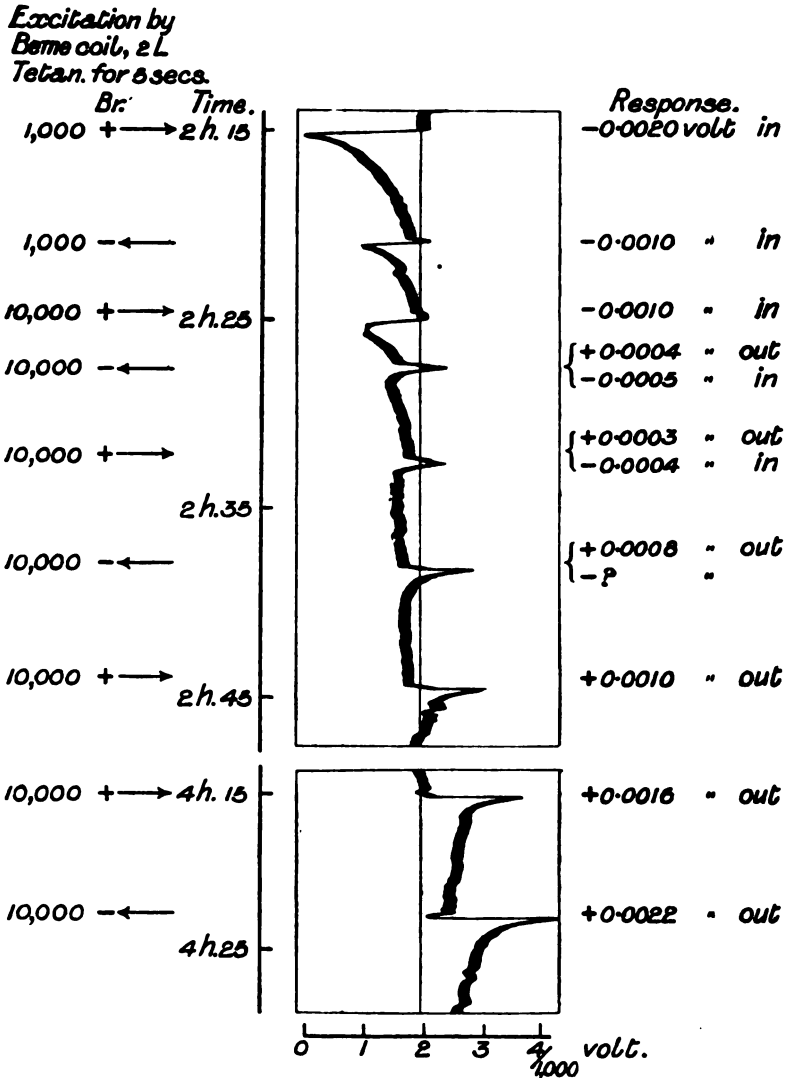
Similar results were observed in other observations of similar type (as well as in experiments according to the A, B, C plan, *vide infra*), weak excitation of the fresh pad giving ingoing, and strong excitation outgoing response. Weak subsequent to strong excitation gave no effect, while the latter gave regular outgoing effects.

Twenty-four hours *post mortem* weak excitation gave no effect ; with strengthened excitation the first visible effect was outgoing, and with strong excitation the outgoing effect was generally larger than on the day of death.

In general the outgoing effect is the far more prominent and regular phenomenon ; the conditions most favourable to manifestation of the less prominent and more evanescent ingoing effect being a fresh skin and not too strong excitation. With lapse of time, or as an immediate consequence of strong excitation, the ingoing effect of weak excitation is abolished.

Record of part of Experiment 9, showing transition from ingoing to outgoing effects, the former by "Weak" the latter by "Strong" Excitation.

FIG. 8. (4235.)



Response to Direct Excitation of the Intact Skin. A, B, C Method.

If two electrodes, A and B, are applied to the intact skin, e.g., to the external surface of two separate pads, and tetanising currents are led in by these electrodes, the subsequent deflection is comparatively small

and of variable direction. Two outgoing currents are aroused at the points A and B, of opposed directions in the circuit, and the deflection under these conditions is only resultant of an inequality in the two forces aroused at A and at B.

In order to investigate separately the local reactions of A and of B consequent upon electrical excitation, of which these two points are poles, it is necessary to employ a third electrode, C, in the following manner:—

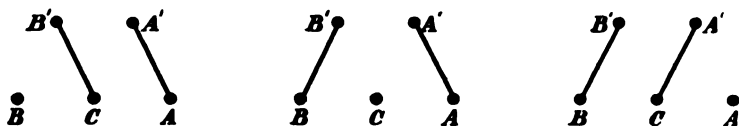
B and A are the exciting electrodes, C is an independent electrode through which no exciting current is passed, and which in conjunction with A or with B is to serve as leading-off electrode to the galvanometer.

If it be intended to examine the state at A after passage of an exciting current through A B, the procedure will be:—1. Compensation of the two points A and C. 2. Excitation through A B. 3. Connection of A and C with the galvanometer.

If it be intended to examine the state at B, the similar procedure will be:—1. Compensation of B and C. 2. Excitation through A B. 3. Connection of B and C with the galvanometer.

The necessary transposition from A to C or from B to C is most readily effected by means of a switch key of the following disposition:—

FIG. 3.



The three electrodes are connected with the terminals A, B, C. A is connected with A' and B with B' by revolving arms, either of which can be turned on to the middle terminal C.

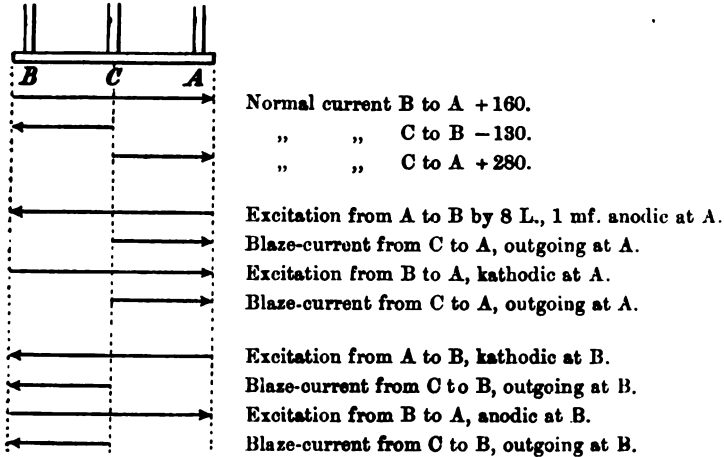
This method is applicable to many cases other than that of the skin. I shall have occasion to refer to it as the "A, B, C Method," and for the sake of brevity shall call the above-described switch the "M switch."

The following protocol will best exhibit the results of a typical experiment and—which is a point of some practical importance—the system on which laboratory notes are taken so as to avoid confusion of direction during experiment.

Experiment 9. Frog's Skin, excited by Single Condenser Discharges.—

Three electrodes, A, B, and C, applied to external surface. Excitation led in through A B. Response led off through A C, and B C, outgoing currents at A and at B.

FIG. 10.



Experiment 10.—Cat's Paw; 24 Hours Post-mortem.

A, B, C as indicated below. Excitation by tetanising currents. Berne coil. Two Leclanchés. 10,000 units. The signs + and - refer to the direction of the break shock.

FIG. 11.

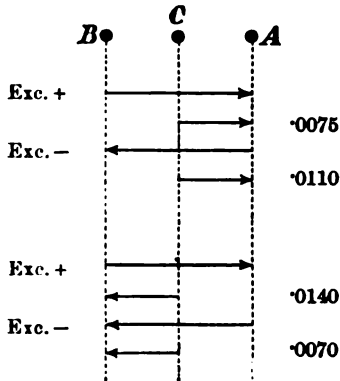


FIG. 12.

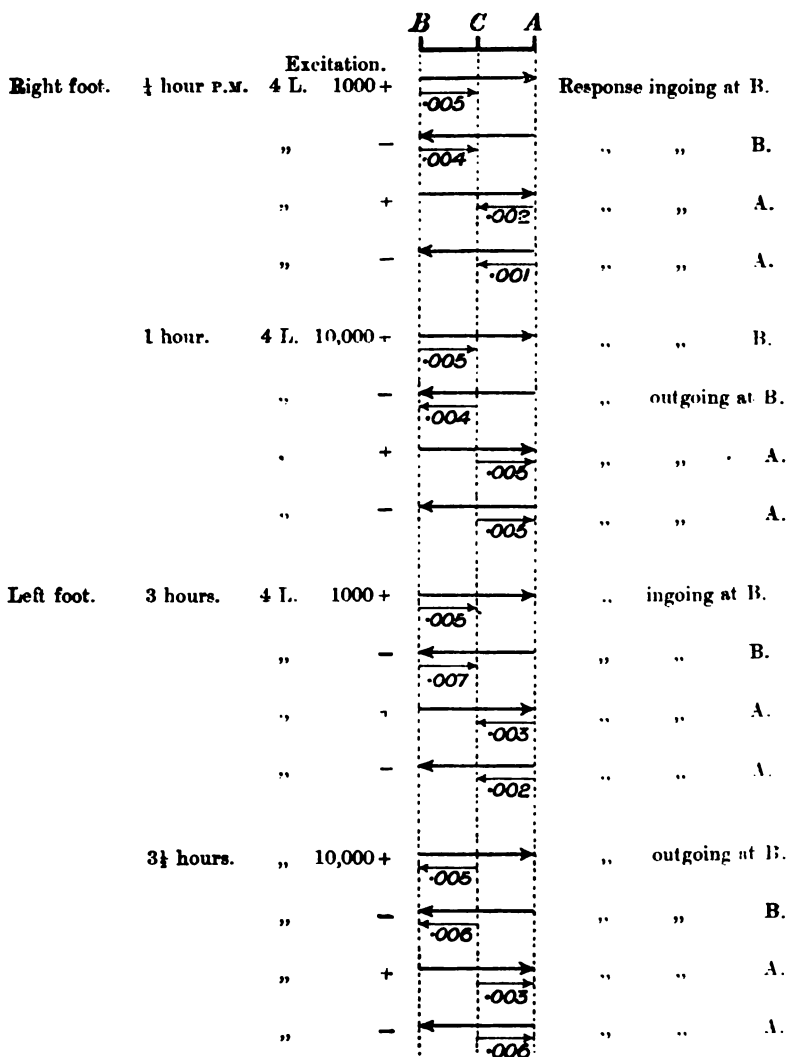


The response is always of the nature of an outgoing current at A, and at B for both directions of excitation.

Experiment 11. Cat's Paw.—Pads excited and led off as in previous experiment. Excitation "weak" and "strong" through electrodes A and B. Response through A and C and through B and C. Ingoing response after weak excitation; outgoing response after strong excitation. Berne coil. Four Leclanchés in primary circuit; tetanisation

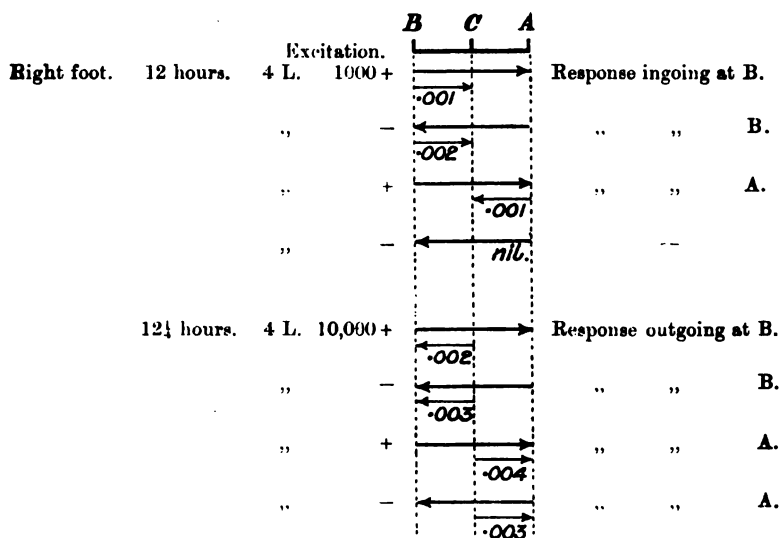
for 5 seconds; + and - signify outgoing and ingoing directions of break currents. Large arrow denotes excitation; small arrow denotes response; numbers below indicate voltage of response.

FIG. 13.



After this series of four trials at 10,000, a series of four trials at 1000 gave no effect. On transfer of an electrode to fresh pad ingoing response observed at 1000.

FIG. 14



After this series of four trials at 10,000, a series of four trials at 1000 gave no effect. On transfer of an electrode to fresh pad an ingoing response was observed after excitation at 1000.

Fallacy to be avoided.

The longest period at the end of which I have detected unmistakable physiological response of a surviving pad has been 7 days *post mortem*. I have not, however, sought to follow out the reaction to its last remaining trace so as to determine a maximum duration of survival, because with declining voltage of reaction the disturbing effects of polarisation counter-currents become more and more apparent. At a comparatively early period the response to single shocks disappears, and the galvanometer, if sensitive, manifests only polarisation effects contrary in direction to the test shocks. At this period tetanisation by currents alternating in direction will still bring out a true summated outgoing effect with both pairs of directions of currents; under these conditions the effect of polarisation is relatively smaller. But at a later period, as the life of tissue is coming to an end, and the response reaches a value to be measured in ten thousandths of a volt, the disturbing influence of polarisation again becomes apparent. In this case the deflection after tetanisation is reversed with reversal of direction, and a resultant due to superior polarisation by make is witnessed in the direction of break. The shortest period after death at which I have observed this state has been

twenty-four hours, in the case of an ill-nourished cat. I regard this as indicating brief survival of tissue.

Fallacies due to anomalous polarisation require to be tested for and excluded. In my experience this kind of fallacy is liable to occur from a defectively amalgamated zinc, and is eliminated by reamalgamation.

I have not thought fit to use tetanising currents with tissue, secondary coil, and galvanometer in one circuit, but have always short-circuited the galvanometer during tetanisation (any current in circuit being of course neutralised by compensation), and non-short-circuited the galvanometer immediately after tetanisation. The time interval in this proceeding has been about 1 second, and upon occasion $\frac{1}{10}$ th second. I have not attempted to ascertain what the electrical state may have been during this transfer-time.

The following experiment is given to illustrate the fallacy of the electrodes and its correction; the explanation offered of that fallacy is only tentative.

Preliminary Experiment to illustrate the Fallacy of the Electrodes and its Removal.

A pair of zinc rods, B A, in a U-tube, connected with a key-board coil, compensator, and galvanometer in the usual way.

The above experiment shows that the fallacy of the electrodes may be of complicated origin, and that care should be taken to ascertain, by separately testing the electrodes, that it has been eliminated. The interpretation of the fallacy that has been given above is purely tentative, and whether right or wrong does not form part of the principal argument, which is based upon experiments in which the fallacy has been eliminated. Nevertheless the fact that it might be present is important to bear in mind, especially in cases where reactions of low voltage (below 0.001 volt) are under examination. But, as previously stated, the fallacy, even if present, would not interfere with conclusions based on reactions of high voltage (above 0.01 volt).

The fallacy of the electrodes is easily recognised and easily avoided. To recognise the fallacy it is sufficient to bring the clay pads of the two electrodes into contact, exactly compensating any accidental current, and then pass testing currents through the circuit, just as when a test object is interpolated between the electrodes. There should be no movement of the galvanometer spot, or at most the slight movement due to defect of compensation or trace of polarisation, in either case less than the movement produced by 0.0001 volt thrown into circuit.

To remove the fallacy, if recognised, it is usually sufficient to carefully reamalgamate the zinc rods. From preliminary experiments with zinc rods, unamalgamated and amalgamated, in water and in saturated solution of ZnSO_4 , it appeared that fallacy of electrodes might be due to a defectively amalgamated cathodic zinc surface rendered electronegative by an exciting current.

Conclusion.

The regular electrical effect of indirect excitation is an ingoing current.

The usual and principal electrical effect of direct excitation is an outgoing current.

An ingoing current may be obtained by direct excitation immediately after death.

I think it probable that both ingoing and outgoing forces may co-exist in the excited skin at the same moment, and that the galvanometric deflection is an expression of this resultant.

This co-existence of two opposite forces is, however, less clearly evident in the case of the cat's skin than in those of the frog's skin and of the frog's eyeball.

November 28, 1901.

Mr. TEALL, Vice-President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair, and the list of Officers and Council nominated for election was read as follows :—

President.—Sir William Huggins, K.C.B., D.C.L., LL.D.

Treasurer.—Alfred Bray Kempe, M.A.

Secretaries.— { Sir Michael Foster, K.C.B., D.C.L., LL.D.
 { Joseph Larmor, M.A., D.Sc., LL.D.

Foreign Secretary.—Thomas Edward Thorpe, C.B., Sc.D.

Other Members of the Council.—Professor Henry Edward Armstrong, LL.D.; William Bateson, M.A.; William Thomas Blanford, LL.D.; Professor Frederick Orpen Bower, Sc.D.; Charles Vernon Boys; Professor William Burnside, M.A.; Professor William Watson Cheyne, C.B., M.B.; Professor George Carey Foster, B.A.; Right Hon. Sir John E. Gorst, M.A.; Professor William Mitchinson Hicks, M.A.; Professor Henry Alexander Miers, M.A.; Professor J. Emerson Reynolds, Sc.D.; Robert Henry Scott, Sc.D.; Professor Charles Scott Sherrington, M.D.; Joseph Wilson Swan, M.A.; and Professor Herbert Hall Turner, M.A.

The following Papers were read :—

- I. "Micro-crystalline Structure of Platinum." By T. ANDREWS, F.R.S.
 - II. "A Comparative Study of the Spectra, Densities, and Melting Points of some Groups of Elements, and of the Relation of Properties to Atomic Mass." By H. RAMAGE. Communicated by Professor LIVEING, F.R.S.
 - III. "On the Properties of the Arterial and Venous Walls." By Professor J. A. MACWILLIAM. Communicated by Sir M. FOSTER, Sec. R.S.
 - IV. "The Development of *Echinus esculentus*." By Professor E. W. MACBRIDE. Communicated by ADAM SEDGWICK, F.R.S.
-

"On the Properties of the Arterial and Venous Walls." By JOHN A. MACWILLIAM, M.D., Regius Professor of Physiology in the University of Aberdeen. Communicated by Sir MICHAEL FOSTER, K.C.B., Sec. R.S. Received October 24,—Read November 21, 1901.

(Abstract.)

The present investigation has included a very large number of experiments, performed at different times during the last 10 years, the blood-vessels of the ox, horse, and sheep being the ones chiefly examined. The conclusions arrived at apply to the vessels of healthy animals.

Post-mortem Contraction.—The occurrence of *post-mortem* contraction in the arteries is described, and its exciting causes (mechanical stimulation, exposure to air, cooling, &c.) stated, and the remarkable persistence of this contraction over many days—in the case of the horse's carotid kept in olive oil over a fortnight, &c.

The influence of chloroform vapour and saline decoction of suprarenal medulla in causing or increasing contraction are described. Also the method of preventing the occurrence of *post-mortem* contraction by freezing, and various methods of removing contraction when it has once been established, by potassium sulphocyanide, ammonia vapour, heating to 50°–55° C., keeping at 38°–40° C. for 24 hours, &c., or merely keeping at room temperature (in blood) for a number of days. The different effects of sulphocyanide on arterial muscle and on skeletal muscle are illustrated.

Very different measurements (as to lumen, thickness of wall, &c.) are obtained from the same artery in contraction and in relaxation.

Effects of Changes in Temperature.—The effects of changes of temperature have been studied in various ways—by causing strips (transverse and longitudinal) of the vascular wall to act upon recording levers (supported by long, feeble, spiral springs) while the strips were heated in an oil-bath, &c.

Relaxed arteries give results very different from contracted arteries, and generally similar to veins—the chief feature on heating being a very marked shortening at 60°–65° C.

Contracted arteries show a definite series of changes when the temperature is raised. Up till two or three days after death there are usually the following changes:—Shortening between 25° and 35°, lengthening about 40°, shortening at 45°–50°, extensive lengthening at 50°–55°, &c., shortening at 60°–65°; a certain amount of lengthening occurs during subsequent cooling.

When the artery is kept longer these changes show a definite order of disappearance; while any contraction remains there is always relaxation at 50°–55°, while at a later stage, when contraction is

absent, there is only the 60° — 65° shortening—with the usual lengthening on cooling. Tracings are given to illustrate these changes. The general behaviour of the arterial muscle is strikingly similar in its general character to what occurs in the bladder (cat), while its muscle is unquestionably alive.

The changes seen in strips from the aorta and the pulmonary artery are slighter in amount (though similar in general character) in accordance with the relatively small amount of muscular tissue in the walls of those vessels.

The effects of heat upon saline extracts of contracted and relaxed arteries are examined and compared with the changes shown by the arterial strips. It is shown that there is no necessary relation between the phases of shortening and the coagulation of proteids in the arterial wall. Coagulation occurs in an acidulated 5 per cent. MgSO_4 extract of artery at 45° — 50° and at 55° — 60° . Experiments were made to ascertain whether proteid coagulation occurred in the tissues of the arterial wall at the same temperatures as in saline extracts.

Contraction v. Rigor Mortis.—*Post-mortem* contraction is evidently a true persistent contraction—very different in its character from the *rigor mortis* of skeletal muscle. This is shown by evidence of various kinds—(1) the effects of stimulation; (2) sulphocyanide; (3) heat; (4) the results of extension of strips of the arterial wall by weights; (5) the behaviour of the artery when distended by internal pressure, &c.

Elasticity of the Arterial and Venous Walls.—This was tested by weighting strips cut transversely and longitudinally from the carotid, &c. Relaxed arteries, as well as veins, gave maximum elongation with the first addition of weight, while contracted arteries showed increments of length increasing up to a maximum and then declining, the curve being first convex towards the axis and then concave—in marked contrast to what is obtained with skeletal muscle and other tissues. When stretched a second time the maximal elongation comes at the beginning.

Strips from the aorta and pulmonary artery behave much like strips from a relaxed carotid.

Both the relaxed and contracted arteries show increased extensibility with light weights when stretched a second time.

Illustrative tracings are given in the paper.

Relation of Cubic Capacity to Internal Pressure.—The portion of artery examined was placed in an oil plethysmograph, and the air pressure in the interior of the artery was raised by means of a system of pressure bottles.

Relaxed arteries were found to be most distensible at low pressures; the maximum increase of volume per unit rise of pressure comes when the pressure is raised from zero, as a rule.

Contracted arteries show increasing increments of volume per unit rise of pressure up to a certain maximum, then diminishing increments. In a strongly contracted artery an enormous pressure (several hundred mm. Hg), vastly higher than the normal blood-pressure of the animal, is required in order to obtain the maximal increase of volume; when contraction is weaker, the level of pressure at which extensibility is greatest is much lower (*e.g.*, 100—120 mm.), and when contraction is feeble the maximal increase in volume may occur at 40—60 mm.; it comes immediately above zero when contraction has been completely abolished, as a result of keeping the artery for a number of days in blood, heating to 40° for a number of hours, or to 50°—55° for a few minutes, freezing for some hours, treatment with ammonia vapour, sulphocyanide, &c.

Both contracted and relaxed arteries show increased distensibility when they have already been distended by a previous rise of pressure.

The main features in the behaviour of the artery are shown by diagrams, and numerical details are given.

Elongation of an Artery when Distended by a Rise of Internal Pressure.—Relaxed arteries elongate vastly more than contracted ones when the internal pressure is raised, *e.g.*, from 0 to 300 mm. Hg.

The character of the tracing is very different in the two cases. In the relaxed artery the maximum elongation per unit rise of pressure comes early—usually with the first rise, *e.g.*, from 0 to 50 mm. Hg.

In the contracted artery the elongation with the first rise is small; it gradually increases with subsequent rises—up to a maximum, which in the case of a strongly contracted artery is only reached with pressures much higher than those stated above.

Tracings are given to illustrate the changes in relaxed and contracted arteries.

Pulsatile Expansion of Arteries.—With a succession of sudden, brief elevations of pressure within an artery the pulsatile expansion of the tube is vastly greater in a relaxed artery than in a contracted one. In the former the maximum expansion is got when the brief rise of pressure starts from zero; the expansion becomes smaller at higher pressures. The observations were made with the aid of a plethysmograph.

Among the most important points described in the paper are—

(1.) The remarkable persistence of vitality in the arteries of a healthy animal—for days after the death of the animal.

(2.) The great importance of the presence or absence of *post-mortem* contraction in influencing not only the measurements of the artery but its behaviour in various respects—

- (a.) Response to changes in temperature.
- (b.) Extensibility of strips of the arterial wall when weighted.
- (c.) Relation of cubic capacity to changes in internal pressure.

(d.) Elongation accompanying rises of internal pressure.

(e.) Pulsatile increase of volume during sudden brief elevations of internal pressure.

The discordant results obtained by various observers (Marey, Roy, and others) as regards the distensibility of arteries, are in large measure explicable by the absence or presence (in varying degree) of *post-mortem* contraction in the arteries they used in their experiments.

“Heredity, Differentiation, and other Conceptions of Biology : a Consideration of Professor Karl Pearson’s paper ‘On the Principle of Homotyposis.’” By W. BATESON, M.A., F.R.S.
Received January 25,—Read February 14, 1901.

In his paper on “Homotyposis,”* of which an abstract appeared in the ‘Roy. Soc. Proc.’ vol. 68, p. 1, Professor Pearson raises an issue of extraordinary importance. In any attempt to perceive the true relation of variation to differentiation, and to analyse the essential similitude existing between Heredity and Repetition of Parts, we reach a fundamental problem of biology. Little has thus far been done towards elucidating this problem or even towards formulating it. The appearance of Professor Pearson’s remarkable memoir may perhaps therefore with profit be taken as an occasion for considering critically some aspects of these questions.

It is impossible to write of Professor Pearson’s paper without expressing a sense of the extraordinary effort which has gone to its production and of the ingenuity it displays. But on careful examination it will, I think, be seen that in the light of known facts there is serious doubt whether the determination of what Professor Pearson calls the average homotyposis of “undifferentiated like parts” can be attained by his observations, and that there is even graver doubt whether, if it was attainable, such a value would have any natural significance. In the course of this consideration it must, I think, also appear that the comparison he attempts between the average homotyposis of “undifferentiated like parts” and average fraternal correlation in families is incorrectly instituted.

At the outset I wish to express the conviction that the leading idea which inspired and runs through the work is a true one. Professor Pearson suggests that the relationship and likeness between two brothers is an expression of the same phenomenon as the relationship and likeness between two leaves on the same tree, between the scales on a moth’s wing, the petals of a flower, and between repeated parts

* ‘Phil. Trans.’ A, 1901, vol. 197, p. 285.

generally. The conception of heredity is thus greatly simplified, and that phenomenon is seen in its true relation to the other phenomena of life, becoming merely a special case of the phenomenon of Division and repetition of parts.

This idea came first to me—as it has perhaps to others—when I was studying the phenomena of Variation in Meristic Series, and in writing on that subject I introduced an outline of the conceptions involved.* On that occasion I ventured to carry this reasoning a step further, as it seemed, and to suggest that *the resemblance which we call Heredity may be a special case of the phenomenon of Symmetry*. The thought then expressed has been a constant companion ever since, and I have become more and more convinced that it is fundamentally true.

I should welcome Professor Pearson's paper inasmuch as it is an attempt—the only one, so far as I know—to emphasise and develop this conception; for, like him, I am sure that it may provide the key to the nature of heredity, perhaps also to problems beyond.

Variation in some of its essential features may thus perhaps be reduced to a geometrical problem. One of the many factors or conditions of fraternal resemblance may be Symmetry of division, quantitative and qualitative. The reference to the phenomenon of Symmetry seemed to me to carry the principle a stage further, and to show Heredity as a special case of a phenomenon, the conditions of which we may reasonably hope, in a measure, to apprehend in at least its simpler phases. Professor Pearson, on the contrary, avoids mention of Symmetry. This arises, I presume, from a desire to use a more general expression, and from a reluctance to appear to exclude from his comparison the relation between members of Linear or Successive Series, whose mutual relationship is not one of Symmetry in the ordinary sense. He would probably prefer to regard Symmetrical division as a phase or perhaps as a consequence of the phenomenon of the production of “undifferentiated like parts” occurring under special conditions.

I still think something is gained by inverting the statement and speaking of the likeness between the parts as a phenomenon of Symmetry. In some respects Professor Pearson's mode of expression is preferable as being more comprehensive, but mine has the advantage of keeping before the mind the fact that it is in the *Symmetry of cell-division* that the *resemblance* between relatives is presented in its simplest form; and also that the axes along which the “like parts” are produced are frequently definite.

Thus, fraternal correlation in its most striking manifestation is seen in the simultaneous variations of Homologous Twins.†

* For a somewhat fuller treatment see ‘Materials for the Study of Variation,’ 1891, Introd. Sect. VII; and also p. 21.

† See ‘Materials,’ pp. 559 and 560. Following the work of Driesch and others

It is true that the geometrical relations of members in Successive Series are not included in the term Symmetry,* but the distinction is largely one of degree, and the transition from one to the other is of frequent occurrence. Similar organs may be repeated in one species in radial series, while in an allied species the same organs by differentiation of an axis stand in succession to each other, as every naturalist knows.

Professor Pearson writes (p. 291): "When we ascertain the sources of variation in the individual, then we shall have light on the problem of fraternal resemblance." May we not also say that when we ascertain the conditions of asymmetrical division we shall have light on the problem of fraternal variation?

I introduce this reference to my own method of expression partly to show how far I am in agreement with Professor Pearson on a main point, and partly to emphasise the significance which the analogy between Repetition and reproduction gains by the reference to Symmetry.

Theoretical considerations in mathematical form are put forward by Professor Pearson as tending to the belief that the numerical value for homotypic correlation will, on an average of cases, approximate to the average value found for fraternal correlation.

The reasoning is beyond me, but I gather that the argument, by the introduction of appropriate assumptions, amounts to a proof that if the characters of the offspring, as measured by their deviations, depend on those of the germ-cells of the parents, then the characters of the repeated parts (or *undifferentiated like parts*) formed by that offspring will similarly depend on those of the germ-cells; and it would then be expected that the correlation between those repeated parts of the same individual would be similar in intensity to that between the germ-cells of its parents. Whether the assumptions are justifiable I am not able to judge, as I do not properly understand them.

The resemblance or correlation between "undifferentiated like parts" is, then, regarded as a phenomenon similar to the correlation between brothers. The latter correlation has been investigated by Professor Pearson in a number of heterogeneous cases, and has been found to vary from .1973 to .6934, where 0 is zero and 1 denotes complete correlation.† The mean value approaches .45. He pro-

on the artificial production of Double Monsters, we must regard the relation between Homologous Twins as of the same nature as that subsisting between the right and left halves of a bilateral organism.

* It would be easy to suggest terms better adapted to the expression of these conceptions, but to do this at present is premature. When it becomes necessary to do so I anticipate that the largely analogous phenomena of rhythmical vibration will provide ready metaphors from which to construct a terminology well adapted to denote the various phenomena of Merism.

† Both here and in the coefficients of "homotyposis" reasons are given for supposing that some of the greater departures from the mean may be explained away.

poses in this paper to find a numerical value for the average correlation between undifferentiated like parts of the same individual. A large series of heterogeneous cases of various organs in various plants have been investigated. The values found range from .1733 to .8607. Reasons are put forward for excluding some of the highest and for doubting the validity of others, especially some of the lower ones. Eventually the average result .45 is again reached, taken on a series ranging from .1733 to .6311.

Professor Pearson attaches importance to the rather close similarity between the two average values. We are bound, therefore, to remark as a suspicious circumstance that the range of values is so wide, and that the average value should so nearly approach the mean of the whole possible range; but upon this point I do not propose to dwell, preferring to deal with more general aspects of the problem.

Now Professor Pearson is attempting to measure to what extent there is a resemblance or correlation between repeated parts of one individual as compared with the same parts of different individuals of the same race: how much, that is, of the resemblance between repeated parts of an individual is due to its individuality? Further, how much on an average of many individuals may be expected to be due to individuality?

For various sources of error Professor Pearson is well prepared. In his *Malva* material, for instance, he finds little correlation due to individuality; because, as he points out, his specimens may have been all or largely the vegetative product of one or few individuals. In some Mushrooms, on the contrary, he finds this correlation high, but he thinks that here individuality may partly be due to stages of growth, for his individuals were not all of similar age. In comparison with what follows these sources of error are trifling.

It will be remembered that the series of homotypes is to be *undifferentiated*. If differentiation exists and is not recognised the apparent homotyposis due to individuality will, as Professor Pearson perceives, be immediately lowered. In order, therefore, that the inquiry should have significance, it is necessary that differentiation occurring between members of a series of parts should have a clear meaning as distinct from variation occurring amongst them; and further, in order that the investigation should be carried through, we must be able to discriminate such differentiation from variation. On critical consideration it will be apparent that neither of these postulates accords with the facts of nature. I cannot find that Professor Pearson has in any real way dealt with this difficulty. The practical difficulty he has perceived and in part met, but the more serious theoretical difficulty has, I think, escaped him. When fully understood, it will surely be seen to invalidate the whole argument.

Let us grant for the moment that differentiation of the parts can be

dealt with—if it can be detected; but if differentiation occurs in different individuals in different degrees and directions, how can it be told whether the ensuing deviations in correlation are due to a change in the control of individuality over the variation, or to irregular and incipient differentiation? Yet, is not such differentiation exactly what is to be expected in the variation of homotypes? Do not most animals and plants exhibit this phenomenon, and must we not believe that these organisms have attained their present forms largely by variations among their repeated parts? In view of these familiar facts, can Professor Pearson point to any feature which positively distinguishes variation occurring between members of a series from differentiation?

That differentiation may in practice be mistaken for variation between homotypes he is aware. It is not, however, the difficulty of recognition I would now emphasise, but the fact that between the two phenomena no absolute distinction exists in nature. An “undifferentiated series of like parts” means only a series of like parts which have varied and are varying among themselves but little. A series of highly variable like parts is a series in which differentiation exists or is beginning to exist in a complex and irregular fashion. A “differentiated series of like parts” means a series among which variation is or has become definite and regular. Between these classes there is every shade and degree. No one can say finally where each begins and ends, and, by appropriate selection, we could find homotypic coefficients of any required value. The *average* value of such coefficients taken at random has no significance in nature.

Let us examine some practical examples.

In Professor Pearson's *Nigella*, for example, the correlation between the numbers of segments in the capsules of individual plants is found to be low. That is to say, given one seed-vessel of the plant, it will give you very little information as to the most probable number of segments in a second seed-vessel of the same plant. Why is this?

From the look of the plant, or, if such simple perceptions are mistrusted, by counting the segments of seed-vessels on lateral branches, and comparing the numbers obtained with those obtained from seed-vessels borne on central axes only, it is easy to show, as Professor Pearson points out, that the numbers are generally lower in the case of the laterals. We recognise, further, that the proportion of laterals varies from plant to plant.

How is the differentiation detected in *Nigella*? By the *regularity* with which small capsules are associated with lateral branches.

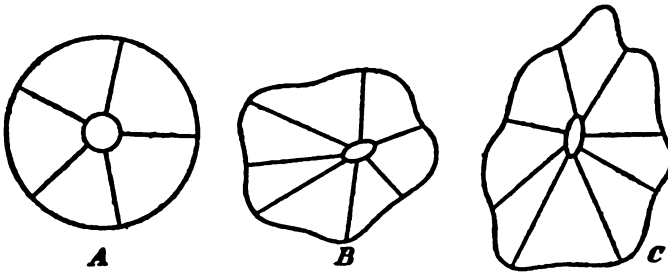
But suppose that for any reason this regularity were masked, should we then perceive the differentiation? Might it not pass, wholly unsuspected, for a change in correlation? Undoubtedly it might.

Take the case of blood-corpuscles of a Frog. Measure some charac-

ter of a hundred corpuscles in each of many Frogs. Find the correlation due to the individuality of the Frog. How can we determine whether in some of the individuals we have used there may not be differentiation such as was found in *Nigella*, so that the parts are not really "undifferentiated like parts"? Would not such irregular differentiation change the mean correlation between the corpuscles?

Would not the fear of such an error practically lead to the exclusion of cases of suspected differentiation *which ought to be included* in calculating the average? Is it not certain that differentiation in important characters may take place in exactly the masked way I have referred to? If, for instance, we could count granules in the corpuscles and work out their homotypic correlation for these numbers, might not we have among our individuals some which had specialised corpuscles absent in others?

Again, does not individuality show itself by *change in the degree* of differentiation among homotypes? Can we frame a definition of variation which will exclude such changes?



A represents a radially symmetrical organism in which we may study the correlation in lengths of the radial septa and determine how much is individual or homotypic, and how much racial. If the radial symmetry were always perfect and the specimens merely of different sizes, the racial and homotypic correlations would be alike, unity. But suppose the population consists partly of (i) approximately radially symmetrical specimens; (ii) quite irregular specimens like B; (iii) of specimens whose forms are controlled by an incipient differentiation of any axis tending towards such a form as C.* How would Professor Pearson's methods determine the true homotypic correlation in this population?

Suppose that in a polychaet, say a Syllid, there is marked differentiation between segments at the anterior and posterior ends separated by hundreds of segments apparently undifferentiated, bearing appendages similarly undifferentiated. We may determine the homotypic

* I have no doubt that a study of the Corals, say, would provide actual examples of such a population. May not some Mushrooms be in just this state?

correlation of these appendages. So long as differentiation regularly and conspicuously begins at a certain region we can exclude it. But suppose in some individuals it begins at one segment and in another at another, as it almost certainly would do, how should we know which specimens or which segments must be rejected as introducing a confusion through differentiation, and which must be included in reckoning homotypic variation? If differentiation is irregular, will it not change the apparent homotypic correlation?

Professor Pearson has determined the racial correlation for the lengths of the first phalanges of digits III and IV in women. It comes out high, .914,* as might fairly be expected by any one who had studied the meristic variations of digits. There is, of course, differentiation between these two digits, so that they may be said to be unsuitable subjects for determination of homotypic correlation of like parts. But if instead of Man, the digits III and IV had been studied in an Artiodactyle, say a Deer, the *racial* correlation would doubtless have been much nearer unity. In other words, these two digits in the Deer are approximately in the relation of bilateral symmetry about the median axis of the foot.

In this case the differentiation between the digits is low. They approach the homotypic condition, and their homotyposis could be measured. But a population may consist of some individuals in which there is a high correlation between these two digits III and IV, and others in which differentiation had begun or sensibly persisted. In such a population the racial correlation would be clearly reduced. But would not the homotypic correlation, as calculated, be changed also? Would Professor Pearson's method show to what extent incipient differentiation had introduced error in the determination of the homotypic correlation?†

Yet another and even clearer illustration. The two claws of a Crab are a pair of homotypes. Their homotypic correlation in respect of any character, length for example, might be determined. Now there are species of crab in which the two claws are approximately equal or undifferentiated. On the contrary, in some species the right, in others the left, in others the right or left with varying frequency, is differentiated in size and other characters. Can it be decided in such a case which deviations from, or approaches to, bilateral symmetry are, *as variations*, to be included in a determination of homotypic correlation, and which are to be rejected as due to changes in differentiation?

On this rather wider view of the facts is it not manifest that the

* 'Grammar of Science,' 1900, p. 398.

† If Professor Pearson declares that such differentiation would be "statistically discoverable," he must assume that the differentiation would always affect the same digit in the same direction, an assumption for which I can see no warrant.

distinction rests on fallacy? The reality of the problem as defined by Professor Pearson depends on the assumption that there is an absolute distinction between differentiation and variation among repeated parts, and its solubility depends on the assumption that this distinction can be perceived. The proviso that such a distinction is to be observed stultifies the whole inquiry. So far are we from being able to perceive this distinction, that we may even doubt whether variation among repeated parts *can* take place except as differentiation. If the idea of variation is to be extended to the case of differences between repeated parts it must inevitably include differentiations among them.

But, unless differentiation can always be detected or always reckoned for, the *average* value of the homotyposis coefficient will have no more natural significance than would the average variability of all organisms measured by their "Standard Deviation" from their various means, or the average size of living cells, or the average weight of all ponderable bodies.

I now proceed to a different point, in a sense the converse of the former. Professor Pearson perceives that the correlation between "undifferentiated like parts" has an analogy with the resemblance or correlation between brothers. But does he recognise that variation between brothers is comparable not merely with variation between repeated parts, *but also with differentiation*, and with predominantly orderly variation among such parts? The phenomena in a colonial or social form will clearly illustrate this principle. Ova and spermatozoa may be treated as "undifferentiated like parts" so long as their variations, judged by the resulting offspring, are sensibly irregular. Can we recognise differentiation among them as distinct from variation? Certainly we *sometimes* can. In determining the correlation of confraternities, the parentage enables us to distinguish the fraternal groups correctly, and consequently a fraternal correlation may be truly determined. *For to do so we are not compelled to distinguish differentiation from variation.*

But I put it that the parallel Professor Pearson is seeking is improperly instituted in his paper. He compares the homotyposis of "undifferentiated like parts" with the correlation between brothers. *He ought to compare it with the correlation between undifferentiated like brothers.* As it is, he is trying to find for homotypes what he would be trying to find if he set about a determination of the average value of fraternal correlation for fraternal groups of *like members taken from families composed of differentiated members.* Such an attempt would immediately necessitate a distinction between differentiation and variation. Had his comparison been correctly instituted, Professor Pearson could hardly have failed to discover the fallacy on which I submit his reasoning is based.

Let me state a case in illustration. In most species of Ants females

are differentiated into workers and queens. Frequently other castes, soldiers and others, are similarly recognisable. As regards formulation of his problem, Professor Pearson will perceive that the parallel to average homotyposis is *not* average fraternal correlation of even all the females from one pair of parents, but the correlation between workers, or between soldiers, &c., of one family. He may reply that this objection, though true on the point of form, can be met by weighting the various castes when they are compared. I doubt whether the difficulty is thus fully met (even if in practice it were possible to carry out the process). Should we even then be comparing comparables?

Should we not still be finding a correlation like that for a miscellany of *differentiated* series of repeated parts?

This reasoning, so clear in the case of Ants, extends to all cases of differentiation between members of confraternities.

To find then a value comparable with the homotyposis of *undifferentiated* like parts we must find the fraternal correlation between *undifferentiated* like brothers. But differentiation has here again no meaning which can be determined with precision. It shades insensibly into variation.

Suppose we merely propose to determine the *average* value of fraternal correlation in *workers* of one genus of Ants. In some species we sort our Ants easily into workers and the rest. In another species we shall find differentiation so imperfect that we cannot say for certain which are soldiers and which workers. Finally, even in the more completely differentiated species we shall find occasional nests (families) which show an imperfect differentiation.* Average fraternal correlation, I think, has no meaning, still less an ascertainable value, in these cases.

† The principle that Professor Pearson calls "homotyposis" I have expressed by the statement that the variations of parts, repeated in series, *may* be "similar and simultaneous."† Beyond this we cannot yet go. Professor Pearson's statement of the principle fails to recognise one of the most important features of homotyposis. Expressed in my own terms, Professor Pearson's "homotyposis" is the principle of "similar and simultaneous variation" restricted to *undifferentiated* like parts.

But relationship is not lost when we pass to the differentiated parts, and such differentiated parts *may* vary similarly and simultaneously with other differentiated parts of the same series, exhibiting the phenomenon of Homœosis. A stamen of a Rose, if it becomes petaloid, is not merely a petal, but a *petal of the individual* Rose it is on. Professor Pearson's principle, as stated by him, misses this point.

If he had correctly instituted the comparison between parts and

* See the writings of Forel.

† 'Materials,' p. 569.

individuals he would have seen this also. For in cases of confraternities he must be familiar with the phenomenon of similar variations occurring simultaneously in separate groups of differentiated members.

But let us now suppose we could define differentiation from variation in general, say, as orderly variation. Even so we could not distinguish it unless its order were conspicuous. In a former paper,* Professor Pearson wrote that "the very nature of the distribution of variation, whether healthy or morbid, seems to indicate that we are dealing with the sphere of indefinitely numerous small causes, which in so many other instances has shown itself only amenable to the calculus of chance, and not to any analysis of the individual instance." As I have on many occasions stated, such a description accords ill with the observed facts of variation. Illustrations to the contrary are numerous and are now becoming familiar; and even in Professor Pearson's later works references to them are not wanting.

Does not, then, the presence of orderly differentiation, in various degrees, *compel* us to an analysis of individual instances? In plain language, we shall have to pick and choose our cases, and the value of our coefficient of homotyposis will depend entirely on how we do it. Has not Professor Pearson himself been so compelled in more than one of his examples, notably in that of *Nigella*? Has he any certainty that such an analysis ought not to have been made in other examples also?

He speaks of the extreme difficulty of determining whether his material is homogeneous in respect of environment, but I miss from his work any deep appreciation of the subtle and evasive quality of differentiation. If any one would obtain a conception of this difficulty let him go to any tree or large plant and set about pruning it, or better, let him try to choose shoots for propagation. Until he tries, it seems simple enough; but when he begins he finds the shoots are of many complexly differing kinds, and unless he has experience of pruning or of propagation, he will not know which to choose. If he studies the tree attentively, he will soon see that the kinds of shoots are largely definite and, in fact, differentiated. The differentiation may be irregular or regular. That of the leaves may or may not be correlated with that of the shoots. The differentiations may be correlated with the age of the wood, with the absolute size of the tree, they may be peculiar to the variety, or they may be individual to the specimen and defy analysis.†

I am of course aware that Professor Pearson knows all this, but I

* 'Phil. Trans.,' A, 1896, vol. 187, p. 255.

† There are examples not only of differentiations occurring irregularly in one species and regularly in another, but also of the separation of these very forms of differentiation as characteristics of distinct varieties. See for instance the heterophyllous Junipers and Cypresses.

gather the impression that he regards these differentiations as largely recognisable and capable of exclusion. He may hope too that by increasing the area of his statistics these orderly disturbances may cancel each other. This appears to me highly improbable.

Order in occurrence is generally the only indication of differentiation, and when the order is obscured, differentiation may pass wholly unobserved. But the presence of such differentiation will vitiate the result, even if the area of statistics be indefinitely increased.

The only answer which seems open is that though it may be impossible to define precisely in words which examples should be reckoned in determining average homotypic correlation, and which must be excluded as showing differentiation, yet in practice the difficulty is not a real one, and that divers features (*e.g.*, regularity of occurrence) enable us to detect sensible differentiation.*

Such an answer is far from covering the whole ground of the objections I have indicated, as may readily be seen by attempting to apply it in the practical illustrations given above. But besides this, to take that ground would be to turn back from that appeal to rigid numerical treatment, which Professor Pearson has told us should be the sole test of these hypotheses.

I may further point out that if it were suggested that the distinction between differentiation and variation may be left to the judgment of the observer; we might by a similar exercise of judgment attempt a distinction of variations into evolutionary or *specific*, and *normal*.

This is far more than a merely logical point. I am disposed to think that such a rough classification boldly made and carried out for a number of familiar forms might greatly promote the study of evolution, even though no precise criterion can yet be provided. This suggestion will be abhorrent to many naturalists, though for want of such a distinction much of the statistical work produced by Professor Pearson and his followers has, I believe, gone wide of its mark, if that aim is the elucidation of Evolution. More fitly might this work be described as "Mathematical Contributions to a Theory of Normality."

In the treatises I have referred to Professor Pearson is seeking for a statistical conception of Species through an examination of miscellaneous variations. The impression left on my mind by such imperfect study of his works as I have been able to make, and especially by the present paper, is that the evidence points to some conception of normality to be otherwise attained, a conception more finite and concrete than any we have yet reached.

By the one word *Variation* we are attempting to express a great diversity of phenomena in their essence distinct though merging in-

* Professor Pearson's reference to *Nigella* (p. 320) as unsuited to his purpose because probably "*unstable*" suggests to me he had here this difficulty in view.

sensibly with each other. The attempt to treat or study them as similar is leading to utter confusion in the study of evolution.*

If normality thus imagined can be shown to be a real phenomenon it is conceivable that we might then profitably attempt to determine in specified cases the average value of homotypic correlation for each case, but the average value for a miscellaneous collection of cases would still have no natural significance.

(Note, added November., 1901.)

On p. 287 Professor Pearson has added a note in which he seeks to meet a part of my objections. He says: "A diversity due to differentiation and a variability due to chance are quite distinct things. The one is the result of dominating factors which can be isolated and described; the other of a great number of small factors, varying from organ to organ, and incapable of being defined or specified. Indeed, upon each dominating factor of differentiation is superposed such a chance variability. Of course all things which differ even by chance variation are in a certain sense differentiated." This welcome passage outlines the conception that must form the point of departure in any attempt to understand variation in its relation to Evolution. The same conception I have myself often laboured to express. On former occasions to these two kinds of diversity of which Professor Pearson speaks I have applied the terms "Discontinuous" and "Continuous." Though useful in practice, those terms are open to misconstruction and perversion. In the present paper I have suggested the nearly equivalent terms "Specific" and "Normal." Similarly, to variations occurring among repeated parts or homotypes we might apply the terms "Differentiant" and "Normal." Throughout nature the variations between the members of fraternities may be discontinuous and specific, and in like manner may the variations between repeated parts be specific and differentiant, though in both classes normal or continuous variations are always superposed on them.

In most cases the naturalist is seldom in much doubt with which he is dealing. But though these two great classes of variation can broadly be recognised and treated as distinct, the distinction may be evasive, and when the differentiation is irregular that distinction must often be obscured and not "statistically discoverable." Professor Pearson is mistaken in supposing that such differentiation must show itself in his seriations. It may appear *only* as a lowering

* For example, in his criticisms (p. 360 and elsewhere) of the view that sexually produced offspring are more variable than offspring not sexually produced, Professor Pearson is merely confusing different kinds of variations and applying to certain kinds conclusions derived from a study of another kind.

of correlation. The diversity due to differentiation may exhibit a "homogeneous chance distribution," as, for example, in my illustration of the crab's claws. We have only to suppose that the "mode" of the population falls on a form with claws approximately equal, and—to take the simplest case—that the frequency of both right-handed and left-handed differentiation is inversely proportional to the magnitude of the differentiation, a state of things common enough in nature.

As a matter of fact even in the case of *Nigella* (p. 320) differentiation was detected not by the seriations, but by common observation. When the differentiation has been once detected, its influence can be seen in the seriations. This is a mere accident. If the material had happened to contain a certain proportion of a second race with a "mode" on 10 or 13 and a secondary "mode" on 8—a condition familiar in plants (from F. Ludwig's beautiful researches)—the differentiation might have been completely masked in the seriations.* As it is, the seriations alone contain nothing which *prove* the existence of differentiation. We happen to know otherwise that high numbers are associated with centrals and lower numbers with laterals. This is not revealed by the seriations. For all they show, the irregular distribution might be due to ordinary discontinuous variation obeying the laws which F. Ludwig has shown such distributions commonly obey.

We can feel nothing but admiration for those statistical methods which, as perfected by Professor Pearson, are yielding many useful results not otherwise attainable, yet their limitations must be constantly remembered. But even if the differentiation could be discovered by these means, in eliminating it we should have arbitrarily excluded a class of facts which ought to have been included in calculating *average* homotypy, or the correlation due on an average of cases to individuality. In determining the *average* correlation between brothers we must bring to account the continuous and the discontinuous alike: so in the *average* of homotypic correlations must be included both the differentiant and the normal alike.

To state the issue in a word: it appears that the attempt to exclude differentiation by definition must constantly fail in practice and is inadmissible in theory.

* I strongly suspect that something of this kind may actually exist in the case of Shirley Poppies.

"Observations on the Physiology of the Cerebral Cortex of some of the Higher Apes." (Preliminary Communication.) By A. S. F. GRÜNBAUM, M.A., M.D. Cantab., M.R.C.P., and C. S. SHERRINGTON, M.A., M.D., F.R.S., Holt Professor of Physiology, Liverpool. Received July 20,—Read November 21, 1901.

(From the Thompson Yates Laboratory, University College, Liverpool.)

[PLATE 4.]

We have been engaged for some time past on inquiry into the physiology of the cerebral cortex of the anthropoid apes. We are able to lay before the Society some new facts regarding the topographical distribution of function in the anthropoid brain. Our experiments have been carried out on individuals representing the four species *Pithecius satyrus* (Orang), *Troglodytes gorilla* (Gorilla), *Troglodytes niger* (Chimpanzee), and *Troglodytes calvus* (Chimpanzee). The specimens so far have included ten adult individuals. Of *Troglodytes niger* one individual used was only a few months old.

I. *Method employed.*

The method of excitation employed for the cortex has been unipolar faradisation, in the manner previously adopted by one of us* in examining the cortex cerebri for ocular reactions. This method allows of finer localisation than that possible with the double-point electrodes ordinarily used. The inductorium (Kronecker's pattern and scale) has been Helmholtzed.

II. "*Motor*" (so-called) *Area.*

This area we find to include continuously the whole length of the precentral convolution. It also enters into the whole length of the *sulcus centralis*, with the usual exception of its extreme lower tip and its extreme upper tip.

In all the animals examined, we have found the "motor" area not to at any point extend behind *sulcus centralis*. Feeble reactions can occasionally, under certain circumstances, be provoked by strong faradisation behind the *sulcus centralis*, but these are equivocal, and appear under conditions that exclude their acceptance as equivalent to "motor-area" reactions.

On the mesial surface of the hemisphere the "motor" area has extended less far down than was expected. It has not extended to the calloso-marginal fissure. Certain areas near that fissure have yielded us movements, e.g., of shoulder, body, wrist, and fingers; but

* Sherrington, 'Roy. Soc. Proc.', vol. 52, 1893.

we hesitate, for reasons to be given in a fuller communication, to class these with those of the "motor" area proper.

We have found the precentral convolution excitable over its free width, and continuously round into and to the bottom of the *sulcus centralis*. The "motor" area extends also into the depth of other fissures besides the Rolandic, as can be described in a fuller communication than the present. The hidden part of the excitable area probably equals, perhaps exceeds, in extent that contributing to the free surface of the hemisphere. We have in some individuals found the deeper part of the posterior wall of the *sulcus centralis* to contribute to the "motor" area.

In the "motor" area we have found localised, besides very numerous other actions, certain movements of the ear, nostril, palate, movements of sucking, of mastication, of the vocal cords, of the chest wall, of the abdominal wall, of the pelvic floor, of the anal orifice, and of the vaginal orifice. We have met with various examples of inhibition effects produced by this cortex, such as described by one of us previously in the cortex of the lower apes.*

We find the arrangement of the representation of various regions of the musculature follow the segmental sequence of the cranio-spinal nerve-series to a very remarkable extent. The accompanying figure (Plate 4) indicates better than can a verbal description the degree of adherence to this sequence.

We do not find that the exciting current for the "motor" cortex requires to be extremely strong for the anthropoid brain. "Epilepsy" is easily evoked from the cortex of the anthropoids.

Our experiments show that the *sulci* in the region of cortex dealt with can in no sense be considered to signify physiological boundaries. Further, the variation of the *sulci* in these higher brains is so great from individual to individual that, as our observations show, they prove but precarious, even fallacious, landmarks to the details of the true topography of the cortex.

[The mere fact that the "motor" area extends in front of but never (so far as our experiments have yet gone) behind the *sulcus centralis*, is but little indication of detailed constancy of relation between the physiological area and even that sulcus, though such a fundamental one; the antero-posterior diameter of the sulcus, being an area often five-and-twenty mm. across, it is, when treated as marking a line on the cerebral surface, but a rough guide for any detailed examination of the functional topography.—November 25, 1901.]

Extirpation of the hand area by itself has been followed by severe paresis of the hand, the hand being for a few days practically useless and seemingly "powerless." In a few weeks use and "power" were

* Sherrington, *ibid.*; also Sherrington and Hering, *ibid.*, vol. 62, 1897, and Hering and Sherrington, 'Pflüger's Archiv,' vol. 71, 1897.

remarkably regained in the hand, so that it was once more used for climbing, &c. The animal ultimately not unfrequently fed itself with fruit, making use of that hand alone. Even small ablations in the precentral gyrus have led to severe though quickly diminishing pareses. On the other hand, ablations of even large portions of post-central gyrus have not given any even transient paresis.

III. *Other Regions of Cortex.*

Our observations indicate that the frontal region, yielding conjugate deviation of the eyeballs, presents such marked differences of reaction from the "motor" area of the Rolandic region that we hesitate to include it with the so-called "motor" cortex; it seems necessary to distinguish it in a physiological category separate from that. Spatially it is wholly separated from the Rolandic "motor" area by a field of "inexcitable" cortex.

As to the occipital lobe, only from the extreme posterior apex of the lobe and from its actual calcarine region has faradisation yielded any movement (eyes), and then not easily.

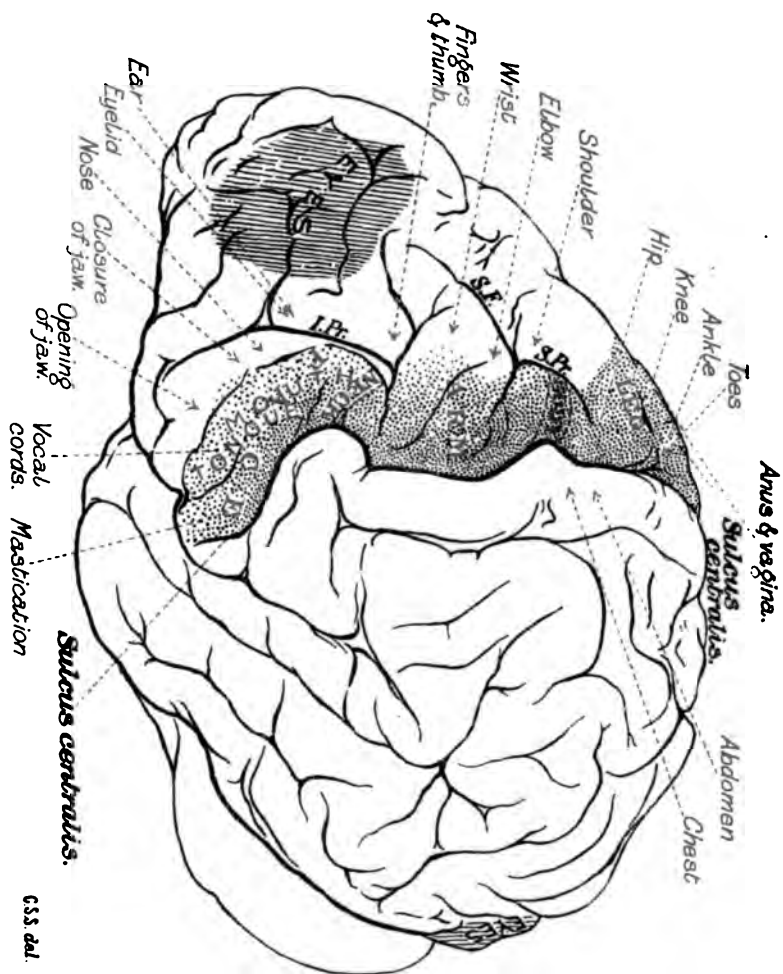
We hope at no long distance of time to be able to lay before the Society a detailed account of the completed investigation. Some of our experiments are still in progress.

It is a pleasure to record here our indebtedness to Dr. L. Mond, F.R.S., for enabling us to bring these experiments to their present stage.

DESCRIPTION OF PLATE 4.

Brain of a Chimpanzee (*Troglodytes niger*). Left hemisphere viewed from side and above so as to obtain as far as possible the configuration of the *sulcus centralis* area. The figure involves, nevertheless, considerable foreshortening about the top and bottom of *sulcus centralis*. The extent of the "motor" area on the free surface of the hemisphere is indicated by the black stippling, which extends back to the *sulcus centralis*. Much of the "motor" area is hidden in sulci; for instance, the area extends into the *sulc. centralis* and the *sulc. precentrales*, also into occasional sulci which cross the precentral gyrus. The names printed large, in red, on the stippled area indicate the main regions of the "motor" area; the names printed small, in red, outside the brain, indicate broadly by their pointing lines the relative topography of some of the chief sub-divisions of the main regions of the "motor" cortex. But there exists much overlapping of the areas and of their sub-divisions which the diagram does not attempt to indicate.

The shaded regions, marked "Eyes," indicate in the frontal and occipital regions respectively the portions of cortex which, under faradisation, yield conjugate movements of the eyeballs. But it is questionable whether these reactions sufficiently resemble those of the "motor" area to be included with them. They are therefore marked in vertical shading instead of stippling as is the "motor" area. S.F. = superior frontal sulcus. S.Pr. = superior precentral sulcus. I.Pr. = inferior precentral sulcus.





Addendum on the Pyramidal Tracts. By C. S. SHERRINGTON.

The spinal degeneration resulting from ablation in the precentral gyrus of the above-mentioned "hand"-area, discovers in the anthropoid cord the human feature of a perfectly large direct ventral (Türcksbündel) as well as crossed pyramidal tract. The relative sizes of these tracts seem about the same as in man.

The homolateral or uncrossed lateral division of the pyramidal tract is also well seen. The crossed pyramidal degeneration from the hand area lesion is clearly traceable down to the lumbar region of the cord. In the lowest brachial segments there is obvious degeneration of fibres in the grey matter of the ventral horn of the crossed side. Some of the large nerve-cells there seem also degenerate.

A lesion at the top of the *gyrus precentralis* gave no ventral pyramidal tract degeneration, and only a very slight uncrossed lateral pyramidal, although an extensive crossed lateral, that descends the whole length of the cord.

"Total Solar Eclipse of 1901, May 17-18. Preliminary Report of the Observations made at Ayer Karoe, Sawah Loento, Sumatra." By H. F. NEWALL, M.A. Received October 30,—
Read at Joint Meeting of the Royal and Royal Astronomical Societies, October 31, 1901.

This expedition was one of those organised by the Joint Permanent Eclipse Committee of the Royal Society and Royal Astronomical Society, funds being provided from a grant made by the Government Grant Committee.

It was originally contemplated that the party should consist of Professor H. H. Turner and myself, and that we should occupy a station inland in Sumatra; but when the former found himself unable to join in the expedition, it was decided not to appoint another observer in his stead, nor to fix upon the station to be occupied until there was an opportunity of learning about local conditions on the spot.

The expedition was to go to Padang, on the West Coast of Sumatra, and to this port I travelled in company with Mr. F. W. Dyson, who went out from the Royal Observatory, Greenwich. It was decided that we should travel out together, prepared either to join forces at one and the same camp, or to form two camps separated by a considerable distance, and our ultimate decision was to depend upon information which we expected to get from the Dutch authorities.

I had communicated with Major Muller, the Chief of the Survey

Department at Batavia, with reference to a possible site for a camp at or near Solok (a station about 40 miles inland on the State Railway), or still further eastwards near the Oembilien coalfields; for it seemed that a station on the east side of the Barisan Mountains would be more likely to have favourable conditions of weather in May than stations on the west side, an opinion based on careful study of both Dr. van der Stok's large work on 'Wind, Weather, &c., in the Indian Archipelago' (Batavia, 1897), and also of the summary published by the Dutch Indies Scientific Society over the names of Major Muller and Dr. Figee, for the information of observers. I may say at once that the reality proved that this opinion, based on averages, was not worth much, for the local conditions seemed, at any rate in 1901, to play a comparatively small part in the determination of weather.

The route chosen for getting to Sumatra was that *viâ* Genoa, and thence by Dutch Royal Mail steamship *viâ* Red Sea direct to Padang, and we are indebted to the Nederland Steamship Company for considerable reduction in fares in consideration of the scientific object of the journey. Mr. Dyson and I and my wife left Charing Cross on March 12, and travelled across the Continent together, and were joined at Genoa by Mr. J. J. Atkinson, who had generously volunteered to go with the expedition to Sumatra to assist in the observations and preparations, as he had done in the previous year with the Astronomer Royal's party in Portugal. We went on board the Dutch mail-steamer "Koningin Regentes," and sailed from Genoa on March 14. The advantages of the route were that it took us in the shortest possible time direct to our port, Padang, lying on the track of the moon's shadow, and gave us the opportunity of making the acquaintance of the Dutch astronomers, who, as we had learnt from Professor Bakhuisen, the President of the Committee charged with organising the Dutch expedition, were going out on the "Koningin Regentes"; and, furthermore, as the boat called at Southampton, it was possible to take the instruments, huts, &c., on the same boat as we ourselves travelled by, without transhipment.

Mr. Dyson and I had seen to sending our cases to Southampton, and had personally put some of the more delicate instruments on board the "Koningin Regentes" on March 4. The large 4-prism spectrograph, which I was to use at the eclipse, was kept in use at Cambridge for observations of the Nova Persei until the day of my departure from Cambridge, March 11, when it was dismantled and packed; it was then taken across the Continent, and put on board the "Koningin Regentes" at Genoa. The Dutch observers, Professor W. H. Julius, Professor A. A. Nyland, and Mr. Wilterdineck, also joined the boat at Genoa, as well as a party from the Massachusetts Institute of Technology, Boston, consisting of Professor A. E. Burton, Professor Hosmer, Professor Harrison W. Smith, and Mr. Matthes.

On arrival at Padang on Saturday, April 6, we made the acquaintance of the Governor of the West Coast of Sumatra and of Major Muller, head of the Survey Department, and Mr. Delprat, the head of the State Railways. The Governor received us on the following Monday morning, and we recall with great pleasure the personal courtesy and friendly helpfulness which he at once extended to us. Nothing could exceed the kindness and consideration which we experienced throughout our stay in the island from the Dutch authorities with whom it was our good fortune to come in contact. I may here add that I took the opportunity, which was afforded me by a visit to Java after the eclipse, to express to the Governor-General of the Dutch Indies the informal thanks of the English observers for the generous assistance which had been given to them in their expedition.

We were indebted to Professor Bakhuisen, the President of the Dutch Committee, for several sets of maps of the admirable survey of the West Coast of Sumatra. The local authorities at Padang added to our obligations by presenting us with free passes for ourselves and freight over all the railway system on the West Coast.

The Governor of the West Coast arranged that Mr. Dyson and Mr. Atkinson should make use of his steam launch to inspect possible sites for a camp on the coast in Trusan Bay, and also on an island 6 miles from the coast opposite Painan. Mr. Delprat was able to give me information and much kind assistance in the matter of sites inland near the Oembilien coalfield, and I at once set off to Sawah Loento with introductions to Mr. van Lessen, the Chief Engineer of the mines.

Thus it came about that when Mr. Dyson and I met again at Padang on the following Thursday, April 11, we had gained such information that we were able to decide at once that we should occupy separate camps (i) Mr. Dyson, together with Mr. Atkinson, on the island Aoer Gadang, assisted by H.M.S. "Pigmy," a gunboat commanded by Lieutenant-Commander Oldham, R.N., which, in response to an application from the Royal Society, had been told off by the Admiralty to render assistance to the observers; and (ii) myself inland, on tolerably high ground, not far from Sawah Loento, the last station on the railway, at the mouth of the Oembilien coal mines, and thus quite on the east side of the Barisan Range. Our stations were thus the most widely separated in Sumatra, Mr. Dyson being at the west end of the line, and the first of the Sumatran observers to be immersed in the moon's shadow, whilst I was furthest inland at the east end of the line.

The rest of this report deals with my own experiences and observations.

It was with regret that I had to decline the invitation of the Boston

observers to join forces with them ; they had chosen an excellent site on a ridge to the south of Sawah Loento, but it was at a considerable distance from the living quarters, and it seemed to me that in that tropical climate we could not afford to spend much of our forces in walking to and fro, for the somewhat extensive programme of observations would require much preparation. Under the guidance of Mr. van Lessen, the Chief Engineer at the mines, an excellent site was found on high ground above the mines to the N.W. of Sawah Loento ; and Mr. van Lessen was good enough to put at my disposal a house close to it, which had recently been built for the use of officers of the mines, but which had not yet been occupied. The house was close to four others in which mine-viewers were living, and, moreover, other houses were being built in their neighbourhood. Hence the conveniences were in every respect very great, for the conditions implied that food supplies would be easily got, water was laid on, and building materials were ready to hand. The house was built on a terrace which had been levelled on the hillside, and I decided to set up the huts and instruments within a few feet of the verandah.

I am very much indebted to Mr. van Lessen for his kind assistance in numberless ways. Not only did he make me free to use the resources of the mines and the coolies employed there, for the purpose of setting up the huts and building piers for the instruments, but he and Mrs. van Lessen helped us in setting up house in rather unusual circumstances, by lending us furniture and in enlisting a similar kindness from the resident "Comptrolleur," Mr. Sieburgh. It is a pleasure to recall the readiness and interest with which the officers of the mines, under Mr. van Lessen's direction, helped us in our needs.

§ 2. *Position of Station.*

The station for the observations was at Ayer Karoe, at the N.W. end of the terrace, on which are built five houses for the overseers of the Oembilien mines. The site is a few hundred yards from the village Doerian, along the road leading from Sawah Loento to Talawi. The position, read off the Survey Map (scale 1 : 20,000), is longitude $0^{\circ} 23' 30''$ E. of Padang, and latitude $0^{\circ} 39' 55''$ S. Major Muller gave the Padang meridian as $100^{\circ} 21' 55''$ E. of Greenwich.

The position of the station may accordingly be taken as—

Longitude $100^{\circ} 45' 25''$ E. of Greenwich.
 Latitude $0^{\circ} 39' 55''$ S.
 Height above mean sea-level at Padang, 1260 feet.

The spot is about 37 miles from the central line and on the north side of it. The duration of totality predicted from the elements adopted by the 'Nautical Almanac' was $5^m 42^s$, instead of $6^m 29^s$ on the central line.

§ 3. *Time Observations.*

From the top of a small hillock within two minutes' walk of the instruments we could see the camp of the Boston expedition near Sawah Loento, at a distance of 2·7 miles (measured on the Survey maps 1 : 20,000). Professors Burton and Hosmer were so kind as to make an arrangement by which we received time signals and so determined the errors and rates of the chronometer and watch which I had brought with me. Professor Hosmer had a white time-ball fixed that could easily be seen against the dark background of the tropical foliage with the help of binoculars; and it was arranged that on any day that we hung out a signal (three white flags) in the early morning, he would if possible drop the time-ball at 11^h 5^m 0^s, again at 11^h 6^m 0^s and at 11^h 7^m 0^s. His time observations for determining the errors and rates of his own chronometers were made with a 2½-inch portable transit and chronograph. Mr. Hosmer gave me signals on four days between the 9th and 19th of May. My watch—an excellent one by Frodsham, made for the 1862 Exhibition—was kept in exceptionally favourable conditions for good time-keeping, and its rate deduced from signals received on May 9 and 16 was + 4·6 seconds per diem, and from May 16 and 19 was + 4·6 seconds per diem.

The rate of the chronometer (by Crisp) was found to have changed from + 0·7 between the 17th and 18th to + 2·1 between 18th and 19th, the chronometer having been moved in and out of the hut several times for the preparatory rehearsals.

The following are the times of contacts observed, those predicted from the elements given in the 'Nautical Almanac' being given for comparison and position-angles of the contacts being also added for convenience of record:—

	Position angle.	Prediction, G.M.T.	Observed.
	°	hr. min. sec.	hr. min. sec.
First contact ..	253	16 5 15	16 5 31
Second „ ..	48	17 39 12	17 39 17
Third „ ..	288	17 44 54	17 44 52
Fourth „ ..	82	19 17 20	Not observed

First contact was observed by me through the declination theodolite, the time of my call "Now" (possibly 5 seconds late) being noted on the Crisp chronometer by Lieutenant Briggs. Second and third contacts are those for Dr. Wallace's signal "Go" at beginning of totality when he was observing the disappearing current on the focussing plate of the Dallmeyer telephotographic camera (image of sun 1·43 inches in diameter) and for my signal "Now" given to Lieutenant

Briggs and Mrs. Newall for the exposures they had to make with their respective instruments for the reappearing flash.

These two observations were recorded in terms of the metronome beats, and were connected with the chronometer readings by my noting on the chronometer the instant at which the last call of the count with the metronome was made by the time-keeper.

The metronome was adjusted to beat seconds the night before the eclipse, and as set it lost one beat in 6 minutes. It was tested about a quarter of an hour before the eclipse, and again after the eclipse, and was found in several trials to lose between one and two beats in 6 minutes.

The photographs of the disappearing and reappearing crescents afford a check on these signals called both by Dr. Wallace and myself, but the discussion is not yet completed.

§ 4. *Meteorological Conditions.*

The following summary may be given of the records kept between April 24 and May 22 :—

Becloudedness.—Out of 23 days on which record was kept of the becloudedness of the sky near the zenith between 12 and 1 o'clock midday—the eclipse occurred at 12.22 P.M.—

1 was absolutely free from cloud and haze (May 1).

4 were free from cloud, but more or less hazy.

8 were completely overcast.

10 were more or less cloudy (including the day of the eclipse).

Temperature and Moisture.—The temperature was recorded for wet- and dry-bulb thermometers in the verandah :—

Time of day.....	6 A.M.		Midday.		Evening (about 9 P.M.).	
Number of days on which readings were recorded	19		11		7	
Thermometer.....	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.
Average	72°	71°	84°	78°	76°	74°
Highest.....	76	74	86	80	77	75
Lowest	69	67	80	74	75	72

Rain.—Records on 21 days :—

On only 2 out of 21 days, on which record was kept, was there rain between 6 A.M. and midday.

On 8 days there was no rain.

On 13 days there was rain in the afternoon or night.

Largest rainfalls recorded were :—

May 8	1·19 inches in 40 minutes.
„ 22	1·72 „ 2 hours.

§ 5. *General Programme of the Observations.*

The operations during the eclipse were to include—

1. Taking a series of photographs of the corona with the Dallmeyer telephotographic combination, in continuation of the series obtained in many recent eclipses, under the direction of the Joint Permanent Eclipse Committee.
2. Taking a series of photographs of the corona with the Savart camera, used at Algiers in 1900, for a study of the polarisation of the corona.
3. Taking a series of photographs of polarised images of the corona with the double-image camera, devised and used by Professor H. H. Turner in recent eclipses.

Nos. 1, 2, and 3 were to be done with the “double-tube” camera, which had been used in many eclipses. Professor Turner had very kindly lent me the apparatus for No. 3, but I had most reluctantly to give up the attempt to adjust it; the weather in the week before the eclipse was unpropitious, and I had to concentrate effort on completing adjustment of instruments already set up.

4. Taking a series of photographs of the spectrum of the chromosphere and “flash” at the beginning and end of totality, with a 4-prism spectroscope with slit, for accurate determination of wave-lengths.
5. Taking a photograph of the spectrum of the corona with the 4-prism spectroscope, two slits being used, to get material for the determination of the relative motion of the eastern and western parts of the corona (rotation of the corona).
6. Taking photographs of the “flash” and corona, with a powerful objective-grating camera, using very high dispersion.
7. Making visual observations of the structure of the green corona ring, as viewed through another objective grating camera.
8. Taking photographs of the spectrum of the flash and corona with a quartz spectrograph giving small dispersion.
9. Making visual observations of the atmospheric polarisation with a view to detecting a change in the plane of polarisation during totality, and for the purpose of getting the information necessary for adjusting the Savart camera used in No. 2 during the eclipse.

§ 6. *Assistance.*

Throughout the preparations for the observations I received most valuable assistance from my friend Dr. L. Wallace, who travelled from Burma to Sumatra especially to help in the work of the expedition. Arriving at Sawah Loento on April 18, he accompanied us to Ayer Karoe, and assisted in every possible way to further the aims of the expedition from the moment of his arrival until all the instruments were packed up again on May 26 after the eclipse. It is a great pleasure to acknowledge my deep indebtedness to him for his friendly and efficient help.

A few days before the eclipse, Lieutenant-Commander Oldham, of H.M.S. "Pigmy," was good enough to tell off Lieutenant H. D. Briggs to come up to Ayer Karoe, together with two petty officers from the "Pigmy," to help in the final preparations, and in the observations. They took part in the preparatory drills and rehearsals, and gave most efficient assistance.

In the observations during the eclipse itself—

Dr. Wallace was in charge of the double tube camera, and gave the signals, watching the diminishing crescent of the sun upon the blue focussing plate of the Dallmeyer telephotographic camera.

Lieutenant H. D. Briggs exposed the plates for the objective-grating camera, after having assisted me in making the final adjustment with the diminishing crescent.

Mrs. Newall made exposures with the quartz spectrograph.

I made exposures with the 4-prism spectrograph, made observations for detecting the polarisation of the atmosphere, and observed the green coronal ring with the visual objective-grating spectroscope.

E. Hobart, Chief Stoker, and H. Taylor, Petty Officer, H.M.S. "Pigmy," counted seconds aloud from a metronome, and handed plateholders.

At 30 seconds before totality Dr. Wallace, watching the diminishing crescent of the sun, and gauging its length by a marked template which I supplied him with, gave a signal to Lieutenant Briggs, and at 15 seconds he called out "Stand by." Hobart began counting seconds with the metronome, until Dr. Wallace gave the signal "Go," when the disappearance of the crescent showed that totality had begun. Then Hobart ceased counting, and Taylor began afresh, and counted steadily on to the end of the third minute; in this count he began each minute with "one," "two," "three," and Hobart called "One minute gone," "Two minutes gone," at each new minute. Then Hobart resumed the counting of seconds, Taylor calling the minutes up to the completion of the sixth minute, it being expected that totality would last for 5^m 42^s, if the prediction based on the 'Nautical Almanac' elements were true, or for 5^m 32^s if the prediction based on a recent

discussion of eclipse observations were true, Dr. Downing, the Superintendent of the 'Nautical Almanac,' having been good enough to write out to us in Sumatra about this result of the discussion. It was arranged that Hobart should continue the counting of seconds after totality was finished, so that I might note on the chronometer the instant at which the final "sixty" was called.

The operations were rehearsed several times on the two days before the eclipse, and everything went off as perfectly as rehearsals could make it. In a later paragraph reference will be made to one unforeseen mishap which arose from the interference of the clouds.

§ 7. The Day of the Eclipse.

When the sun rose and the morning mists were dispelled on the 18th—the day of the eclipse—the conditions seemed vastly more favourable than on any day in the previous week, during which it must be confessed we had rather a trying time, inasmuch as sunshine was needed for the final adjustment of the two spectroscopes, and it was very difficult to find moments between clouds when enough could be got.

The early hours of the morning of the 18th were occupied in completing the adjustments, which, as it turned out, were already nearly perfect, except in the case of the quartz spectrograph, as will appear in a later paragraph. The clocks for driving the celostat and the large spectrograph were finally rated, and we had gathered for a last rehearsal, more for the sake of establishing confidence than of adding to the instruction and drill, which had been gone through many times on the two previous days. But about 11 o'clock, very soon after I had observed the first contact, clouds began to move up from the N.E., and to form round the sun, and it began to be an anxious time. I was content to dispense with the rehearsal in dumb-show, and thus avoided the risk of inadvertent exposure of the photographic plates in the dark slides, preferring to give some finishing touches to the arrangements for signals, exposures, and observations. Nothing was lost by this procedure, and in the actual event the programme was perfectly carried out by everyone concerned, except that cloud interfered in an unforeseen way (see § 11, p 221) with my operations with the 4-prism spectrograph, thus curtailing my most important exposure by $2\frac{1}{4}$ minutes, and also involving the loss of one of the exposures with the double tube.

Fifteen minutes before totality the cloud was still hanging about, and obviously forming in some places and dispersing in others. Iris-coloured rings were seen round the diminishing crescent, showing that the clouds were water-clouds, not ice-cirrus. Ten minutes before totality I looked into one of the spectrographic instruments (see

§ 16, p. 231) and failed to distinguish in the dispersed image the light contrast between crescent and cloud. Five minutes later I exposed a plate in the 4-prism spectrograph, using the light reflected from the clouds near the sun; the resulting negative has unexpectedly great density. Four minutes before totality I was able, through a gap in the cloud, to adjust the visual objective grating (see § 17, p. 233), and 2 minutes later to perform a similar adjustment for the photographic objective grating, an operation which 8 minutes earlier I had given up as hopeless. The cloud was a sort of mackerel cirro-cumulus, giving momentary clear views of the crescent, which was more generally partially obscured by the flocculent masses. As the sunlight faded with the waning of the crescent, the visibility of this veil of cloud decreased, but its presence was clearly indicated by the coloured rings.

Cloud continued in greater or less degree throughout the whole of totality, as will be gathered from the record, given in the following paragraphs, of the observations made with the various instruments.

§ 8. *Huts for the Instrument and Dark Room.*

Two huts—wooden framework and Willesden canvas—were taken out to Sumatra.

One was 15 feet 2 inches long, 10 feet 10 inches wide (the length of the sides being multiples of 2 feet 2 inches, the width of the canvas), 6 feet high at the eaves, 8 feet 6 inches high at the ridge. The ridge-pole was not central, but 4 feet 2 inches from one side and 6 feet 6 inches from the other; two canvas-covered doors were provided, one in each long side, at the ends of a diagonal, and the framework of the hut was covered over with Willesden canvas, except over about two-thirds of one side of the roof. To cover this part six thin sheets of the lightest galvanised iron (6 feet by 2 feet, 10 lbs. to a sheet) were used as a roof which could be easily taken off when it was desired to make observations or to let in light and air into the hut, and which, when replaced, formed a water-tight roof that resisted even tropical rain. Over the hut, and quite separate from it, a light bamboo framework was erected by native workmen, and a thatching of rough grass ("alang-alang") was fitted over it, leaving only a space of a foot and a-half between it and the canvas roofing; the thatching was, of course, omitted over the galvanised-iron sheets. This hut was used to cover all the instrument, and was found extremely convenient.

The other hut was 10 feet 10 inches long, 8 feet 8 inches wide, 6 feet high at the eaves, 8 feet 6 inches high at the ridge. It had been used in Algiers for the instrument hut; on the present occasion it was converted into a photographic dark room. For this purpose a roll of *Willesden waterproof paper*, 5 feet wide, ordinarily used for under-slatting, had been taken out to Sumatra, and was used to make the

canvas hut light-tight. The canvas was put on outside the framework, and the roof was thatched with "alang-alang" on the top of the canvas, to help to keep out rain and sun, and the thick paper was tacked in position inside the framework. This was done in such cunning fashion under Dr. Wallace's direction, that the hut was well ventilated, and I doubt if so convenient and comfortable a dark room has ever been constructed in the tropics.

With reference to the dark room, I may add that a good supply of ice for keeping the chemical and washing baths cool was obtained from Padang, but this was only used for the plates that had to be preserved. A great many of those taken in the process of adjusting the instruments were entirely rotted when ice was not used, but as these were only of passing value there was no object in preserving them.

§ 9. *General Arrangement of the Instruments.*

Inside the large hut three piers were built :—

- (i.) One to carry the 4-prism spectroscope and the visual objective grating ; top level and oriented north and south.
- (ii.) Another immediately to the west of it to carry the cœlostat ; top level and oriented north and south.
- (iii.) A third, still more to the west, to carry the double tube camera, the photographic objective-grating camera and other apparatus, all inclined and pointing downwards towards the cœlostat ; top inclined at an angle 22° with the horizon, and in azimuth 70° west of south.

The hut was set so that one diagonal of the base was parallel to the length of the third pier, or in other words, to the double tube camera ; the long sides of the hut thus lay nearly east and west. The part of the roof over the N.E. corner was covered with the movable corrugated iron, so as to admit the sunlight to the cœlostat and 4-prism spectroscope below.

The two clocks for driving the cœlostat and the 4-prism spectroscope were put on piers just outside the north side of the hut, and were protected by sheets of corrugated iron leaning against that wall of the hut. The weights for the cœlostat clock were hung over a pulley attached to the bamboo framework that supported the thatch.

The visual polariscopes were fitted to the framework of the hut up in the N.E. corner.

§ 10. *The Cœlostat.*

The 16-inch cœlostat, which was used by Professor Turner in India in 1898, and in Algiers in 1900, had been specially provided, by the care of Dr. Common, with an additional support to the mounting, which made it quite stable and suitable for use on the equator. The

mirror, made by Dr. Common and silvered by the Improved Electric Glow-Lamp Company, had a very fine surface, which was found in admirable condition when it was unpacked and put into its mounting on May 14. All the preliminary adjustments of the polar axis of the instrument had been made by means of the attached declination-theodolite, as in recent eclipses. Dr. Common kindly lent me a 6-inch flat, silver-on-glass mirror (also silvered by the above-named Company), and I used it once or twice before May 14, temporarily fitted to the mounting of the *cœlostæt*; for it seemed safer in the tropical climate to run as little risk as possible with the 16-inch mirror, though I had, thanks to the kindness of Mr. Hinks, taken out silvering solutions in case of need. The final adjustment of the polar axis of the instrument was completed after the large mirror had been put in position in the mounting, and calls for no special mention.

The tilt of the mirror—*i.e.*, the perpendicularity of its normal to the polar axis—was tested by making the polar axis horizontal, as judged by the readings of the level on the theodolite, and then comparing the readings of a small level carefully placed on a sheet of paper on the silvered surface of the mirror, with the readings of the theodolite level. This procedure was regarded as sufficient and satisfactory for a place close to the equator, though exception might be taken to it were it adopted in higher latitudes.

§ 11. *The 4-prism Spectroscope with two Slits.*

It was intended to attempt—

- (i.) To secure five photographs of the bright-line spectrum of the sun's limb at the beginning of totality, and six others at the end, one slit only being used.
- (ii.) To photograph the spectrum of the corona in both eastern and western equatorial regions to get material for determination of the relative motion in the line of sight (rotation of the corona), two slits being used.

The instrument arranged for this work is a 4-prism spectroscope with two slits. It was used by the writer in India at Pulgaon in 1898,* and at Algiers in 1900.†

The train of prisms is of such dimensions and construction as to transmit a 2-inch beam of light and to produce a minimum deviation of 180° for H_γ . The camera and collimator are set parallel to one another.

The whole spectroscope is mounted so as to turn about an axis

* 'Roy. Soc. Proc.,' vol. 64, p. 55; 'Monthly Notices, Roy. Astro. Soc.,' vol. 58, App., p. [55].

† 'Roy. Soc. Proc.,' vol. 67, p. 356; 'Monthly Notices, Roy. Astro. Soc.,' vol. 60, App., p. [20].

parallel to the collimator. The axis is rotated (with a period of 24 hours) by clockwork, and is tilted so as to be parallel to the earth's axis. It was adjusted so that the collimator pointed to the south pole and the camera to the north pole.

The tube of the collimator is prolonged beyond the plane of the slit, and is arranged to carry at its end a mirror of speculum metal and an object-glass, by means of which an image of the sun can be thrown between the two slits.

The whole arrangement thus consists of a spectroscope combined with a polar heliostat, and in virtue of the fact that the spectroscope is rotated together with the mirror, the image of any celestial object thrown on the slit does not rotate relatively to the slit. The mirror is mounted in such a manner that the axis about which it is tilted can be oriented relatively to the collimator tube, so that the slits may be set parallel to any diameter of the sun. On the present occasion the slits were set parallel to the axis of the sun.

In India the slits were set at too great a distance apart, and the light of the corona was not strong enough to impress the photographic plate; the slits were set so as to catch the corona nearly 8' from the sun's limb on each side. In Algiers the instrument was used with only one slit, and the spectrum of the corona was photographed in two regions to a distance $3\frac{1}{4}'$ from the limb with an exposure of 49 seconds, and the resulting negative was a good strong one. In Sumatra I expected to be able to get an exposure of 270 or even 300 seconds, and to avoid risk of disappointment the slits were set at such a distance apart as to catch the corona 3' from the sun's limb on each side. This distance was as small as it seemed wise to adopt, account being taken of the apparent diameter and movement of the moon during totality.

The instrument was fairly well adjusted on May 2, and I then took the opportunity afforded by a clear night to throw an image of the full moon on to the slits. It was brightly seen and easily adjusted; but to make matters more certain, I painted the slit-plate white, so that there might be no possible difficulty in setting the image of the corona between the slits after the exposures had been made for the "flash" spectrum with the crescent across one of the slits. The sequel will show that even this precaution was not enough under the circumstances.

The adjustment of the axis of the instrument to parallelism with the earth's axis was accomplished in the same way as in India and Algiers by means of a theodolite with declination circle and level, which was attached to a part of the frame of the spectroscope specially prepared for it.

The rotating plate-holder, charged with twelve small plates, was again used for the exposures as in Algiers.

I was observing the diminishing crescent through the guiding eye-piece of the spectroscope when Dr. Wallace called out "Stand by," and at once hastened to the plate-holder to make exposures for the "flash." Five exposures had been made, each twice as long as I had intended to make had there been no cloud, and as the counting went on longer than I anticipated I decided to make one more exposure, thus using a sixth plate at the beginning of totality. Immediately after having moved Plate No. 7 into its place with shutter closed, I returned to the guiding eye-piece to set the corona ring between the slits. I could see one side of the faint ring distinctly, but could not see the slits, but having learnt by experiment in the course of the morning what movements of the declination and R.A. slow-motions were required to move the image from one position to the other, I made those movements and, being still unable to see the slits, looked up at the corona. There was no difficulty in seeing the dark moon, so to speak, but the corona was quite indistinct beneath the cloud, and was surrounded by a colourless bright ring, which is referred to in the "General Remarks." My notes, written directly after the eclipse, contain the following:—

"I heard count 'Thirty,' and took a candle lamp [which was standing in the tent] to try to illuminate the slit-plate, but failed to get light in. Then, as 'Thirty-five' was called, I turned to polariscope. No trace of polarisation. Turned to second polariscope and could see no trace of polarisation, but saw mackerel sky very strong as it seemed, all over region near the sun, especially to N.E. . . . Returning to 4-prism spectroscope I found the whole ring of the corona visible, but still could not see the slits. Tried candle again; no use. 'Two minutes gone' was called. Ring was clearly lighter, but slit not visible; cloud pretty strong over corona, which could only be traced about half a diameter from the limb. I walked towards double tube and met W. [coming back into hut], who was I thought waiting for improvement in cloud. He asked, 'Is Savart set.' . . . [I replied 'Yes' and] he at once exposed plate-holder No. 2 in the double camera, and I heard '2^m 35'' called as I walked back to spectroscope. Corona was brighter, and I found slits visible, and moved corona ring on to the right place and exposed at 2^m 45' [having lost two minutes and fifteen seconds, exactly half the intended exposure]. Then I turned to polariscope and still failed to see any bands, and then went to visual grating . . . Clouds seemed thinner when I looked up at sky, and Mercury and Venus were very bright. I had one more look at corona on the slits of 4-prism [and found it still correctly adjusted on the slits, and the clockwork evidently driving well], and then as Briggs came back into hut, I looked at polariscope for third time and again failed to see any bands on sky. At 5^m 0' I saw B. and Mrs. N. close their exposures, and I closed mine at 5^m 3', the rubber band of

shutter sticking rather. Then re-set bright side of corona according to picture [previously made to indicate position angle of crescent on slit], and began to expose the five remaining plates in 4-prism [for second 'flash']. At 5^m 35^s I called 'Now' to Briggs and Mrs. N. for their final photographs, when I felt end of totality had come . . . [I turned to chronometer and waited for] W.'s signal 'Now,' which was to mean the 15-seconds crescent [of returning sun]. I noted this on chronometer, and waited further for Hobart's final 'Sixty,' which I also noted on the chronometer. Called 'Cover slides,' and then went out of hut and saw mackerel cloud all over the sky near the sun and 'glory' rings coloured round crescent. Sky got rapidly more clouded, and clouds drifted slowly towards west. Collected notes from observers."

Summary of Exposures.—Six exposures were made near second contact with the following durations :—

1 second, 1 second, 2 seconds, 2 seconds, 2 seconds, 3 seconds.

The seventh plate was exposed for the corona spectrum for 2^m 18^s. Five exposures were made near third contact with the following durations :—

2 seconds, 2 seconds, 1 second, 1 second, 1 second.

Results.—Of the twelve plates thus exposed, only four plates show any impress of the light. The rest, including the exposure on the corona, show nothing, though development was pressed as far as possible.

Five minutes before totality a photographic plate was exposed for 15 seconds to get the spectrum of the sunlight reflected from the clouds near the crescent—this was to serve to give standard measurements to which the measures of the corona spectrum were to be referred—and a good strong negative was obtained.

The plates at second contact are very feeble negatives, and I attribute their weakness to cloud. Photographs obtained with the other instruments at the beginning of totality show that at least three-quarters of the light was intercepted by cloud.

The spectrum of the corona failed because of insufficient exposure under the cloudy conditions.

The plates at the end of totality, when there was certainly very much less cloud, show no sign of impress of the light, and I can only explain it by assuming that the image of the reappearing crescent must have been set across the slit in such a way that three or four more exposures than I had the means of giving were needed to catch the reappearing chromosphere. Photographs obtained with the objective grating show that there was plenty of light to impress the plate, if only it had passed into the slit.

§ 12. *Dallmeyer Telephotographic Camera.*

The double camera used by Professor Turner at Sahdol in 1898 and at Algiers in 1900 was used on the present occasion in the modified form adopted at Algiers. One of the 7×7 -inch tubes contained the photoheliograph objective No. 2, of 4-inch aperture and 5-feet focal length, with a Dallmeyer secondary magnifier of $7\frac{1}{2}$ inches focus, placed 5 inches within the focus, giving an image of the sun 1.43 inches in diameter on plates $6\frac{1}{2} \times 6\frac{1}{2}$ inches.

The method adopted for focussing was first to focus on trees on a distant hill top, viewed through blue glass, and then to use the collimating process, adopted in many recent eclipses. A plane mirror set perpendicular to the axis of the camera reflects rays emerging from the objective back again through the objective; the image of objects in the focal plane is thus formed in the focal plane. The mirror used on the present occasion was the 6-inch kindly lent to me by Dr. Common, and a negative, specially prepared with an image of a finely graduated scale in white on a black ground, was used as the object to be focussed on a sensitive plate, which was put in the same plane with it. Four trials gave sufficient data to set the focus accurately.

The results obtained in the eclipse are as satisfactory as the lenses will give, and it seems right to suggest that some other lenses be used, if it is decided to take this kind of photograph of the corona in future eclipses.

The programme of exposures with this camera was of necessity the same as that adopted for the polariscopic images referred to in § 13, p. 227, for the exposures were made by means of the one shutter for both cameras. The six slides were filled with Ilford Empress plates, and all six were to be exposed during totality if circumstances allowed. It was intended, if the sky was clear, that the exposures should include the series, $2''$, $4''$, $8''$, $16''$, $32''$, and another long one ($120''$) with diminished aperture; and in case of cloud the long exposure was, if necessary, to be sacrificed altogether, and the exposures in geometrical series were to be secured, if possible, in clear intervals. The order of the exposures was to be:—

1. 8 seconds, followed by an interval in which the Savart camera would be set by me after visual observations of the atmospheric polarisation had been made.
2. 16 seconds.
3. 32 seconds, after which Dr. Wallace was to put a cap on the lens to diminish its aperture.
4. Long exposure, to be sacrificed if necessary; after this the cap was to be removed again from the lens.
5. 4 seconds.
6. 2 seconds.

In the event, the long exposure was sacrificed, the other five were made.

The actual exposures were :—

1. 8 seconds, beginning 10 seconds after the signal "Go."
2. 17 " " 2 minutes 35 seconds after the signal "Go."
3. 36 " " 3 " 15 " (1 ± 5 seconds).
4. Omitted.
5. 4 seconds, beginning 5 minutes 3 seconds after the signal "Go."
6. 2 " " 5 " 18 " " "

The first exposure was made through clouds.

The interval between the first and second exposures was more than 2 minutes longer than it would have been had the sky been clear. In the third exposure one of the shutters had evidently not acted, the photograph showing nothing but the standard squares, which had been put on just before development. The last two exposures were carried out according to programme, and were made probably at moments when the sky was clearer than at any other time during the whole eclipse.

The Standard Squares.—After the eclipse, Sir. W. Abney's standard squares were impressed on all the plates for photometric measurements. Exposures were made to a standard candle at 5 feet from the plate, and, following a suggestion made by Professor Turner after his experience with the plates taken at Algiers in 1900, two sets of squares were impressed, with different exposures, on all the plates, as follows :—

Plate.	Exposure for corona.	Exposure for standard squares.	
No. 1	8 seconds	10 seconds and 5 seconds	
" 2	17 "	20	" 5 "
" 3	36 "	80	" 5 "
" 4	not exposed	80	" 5 "
" 5	4 seconds	80	" 5 "
" 6	2 "	20	" 5 "

Development.—All the plates were developed with metol.

When it was found that there was no image on No. 3—one of the exposing shutters evidently had not acted—it was thought wise to impress squares on No. 4, and develop it, just for the very improbable chance that it had been exposed in mistake for No. 3, but no image appeared on the plate.

Results.—The four photographs obtained are all good clean plates. The effect of the cloud is clearly seen in the plates, though it is surprising that the definition has not been more affected by it. The first exposure (8 seconds) shows less than the last (2 seconds);

and the second (17 seconds) shows very little more than the fifth (4 seconds).

The form of the corona is clearly of the type associated with the minimum in the sunspot cycle.

The eastern equatorial region shows great disturbance, and there are many striking prominences on the eastern limb and several marked arches. In particular, reference may be made to (i) two dark rifts radiating from the neighbourhood of a prominence near the sun's equator, on the east side; (ii) bright and dark arches in no less than three of the quadrants, extending about 4' from the limb; (iii) a dark region surrounding a large prominence whose position-angle is approximately 20° measured from the N. point of the sun; and (iv) a remarkably definite edge as of a chromospheric layer for a considerable arc of the sun's limb, in the N.W. quadrant, such as Sir Norman Lockyer described as seen in the photographs secured in Spain last year.

A summary is given below of the extension noted in the different photographs—extension being measured beyond the *moon's* limb, and expressed in minutes of arc:—

	Plate No. 1. 8 seconds.	No. 2. 17 seconds.	No. 5. 4 seconds.	No. 6. 2 seconds.
North polar streamers	5'	15'	21'	5'
East equatorial	8	23	19	8
West „	5	22	12	9
South polar	3	10	8	4

§ 13. *The Polariscopic Camera (Savart Plates and Nicol Prism).*

The Savart camera, which was constructed for use in Algiers, was to be used on the present occasion with a view to getting information as to the amount of polarisation in the outer regions of the corona, and, in particular, in the search for the position of maximum polarisation.

Professor J. J. Thomson very kindly put the large Nicol prisms from the Cavendish Laboratory collection at my disposal again, and the Savart plates were arranged in the same way as last year.*

The camera was put in the second compartment of the double tube, side by side with another camera which was provided with a similar lens, but had no polarising apparatus in front. In this way images of the same scale were obtained side by side on the one plate, one image being taken through the polariscopic camera, the other through the plain camera, each camera having an aperture approximately F/27.

* 'Proc. Roy. Soc.,' vol. 67, p. 363; 'Monthly Notices, Roy. Astro. Soc.,' vol. 60, App., p. [27].

The cameras were focussed on trees on a distant hill viewed through blue glass, and gave satisfactory images.

The programme of exposures was the same as that for the Dallmeyer telephotographic camera, as follows :—

8 seconds, 16 seconds, 32 seconds, 120 seconds, 4 seconds, 2 seconds, thus including one long exposure, and five others in geometrical progression, it being intended to compare the polarised image on one plate with the unpolarised image on the plate next before it in the series. The Savart was to be set in accordance with the result of visual observations of the atmospheric polarisation, so that the bands due to atmospheric polarisation should be obliterated. The first photograph was made with an arbitrary setting of the Savart, and the setting chosen was one that would give the coronal bands slightly inclined to the sun's equator. In the event no atmospheric polarisation was detected, and the Savart was left at the same setting throughout all the exposures. Mr. Wallace made simultaneous exposures for these and for the Dallmeyer telephotographic camera.

Results.—As in the case of the Dallmeyer telephotographic camera, four photographs were obtained, standard squares being impressed on each plate after the eclipse with a standard candle at 5 feet.

	Exposure for corona.	Exposure for standard squares.
No. 1	8 seconds	10 seconds and 5 seconds
„ 2	17 „	20 „ 5 „
„ 5	4 „	20 „ 5 „
„ 6	2 „	— „ 5 (one only).

All the photographs with the Savart camera are good clean plates and show marked polarisation of the corona. The following table summarises the extensions of the coronal streamers in the unpolarised images and of the bands in the polarised.

Extensions expressed in Minutes of Arc from Moon's Limb.

P = Polarised Image. U = Unpolarised.

Plate.....	No. 1.		No. 2.		No. 5.		No. 6.	
Exposure	8 seconds.		17 seconds.		4 seconds.		2 seconds.	
	U.	P.	U.	P.	U.	P.	U.	P.
North polar streamers	8'	P	20'	28'(?)	15'	10'(?)	8'	?
East equatorial	13	57"	46	69	29	28	15	10'
West „	8	49	41	97	29	34	20	23
South polar	5	P	16	?	11	?	8	?

In estimating extensions in the unpolarised images no account was taken of light, which it seemed fair to attribute to diffusion from clouds. Extension was gauged along *streamers*, not in structureless luminosity. In many cases bands can be traced in the polarised images further than streamers in the unpolarised.

Many years ago Sir G. G. Stokes pointed out that the Savart polariscope might be used for the detection of faint extensions of the corona, and it may be that these photographs give evidence of the value of that suggestion.

Across the dark moon very faint bands are detectable only in the 17-second exposure. I am not yet prepared to say how far the discussion of the results may lead me to attribute the extension of the bands to the diffusion of polarised light which emanates from the corona by terrestrial clouds, but it is clear that the resultant polarisation of such light must be, so far as the Savart bands are concerned, in a plane parallel to the equatorial extensions.

The value of the photographs, *as a series*, with related exposures is much interfered with by the variation in the effect produced by cloud, and in consequence it will hardly be possible to utilise them for finding at what distance from the sun there is the maximum relative polarisation, even were the impressed extensions great enough. But as each plate had two exposures made on it simultaneously, one of the polarised image and one of the unpolarised, it is anticipated that they will serve well for the determination of the amount of polarisation in many regions of the corona.

The accompanying figure (fig. 1) indicates the contours of equal brightness in the two images, polarised (dotted line) and unpolarised (unbroken line) obtained in the second (the longest) exposure. It is reduced from a drawing made on a screen on which the two images were successively thrown by means of a lantern. The most marked streamers in the corona are indicated as well as the contours. No great precision has been aimed at in the diagram, its object being merely to give an idea of the sort of material obtained for the investigation. The phenomena are somewhat complicated; in consequence of the radial polarisation of the corona, the bands in the quadrants through which the central band passes form a system with white centre, those in the other quadrants form a system with dark centre; the two systems meet along lines passing through the centre and inclined at 45° to the central band. Hence some care must be exercised in interpreting the appearances presented.

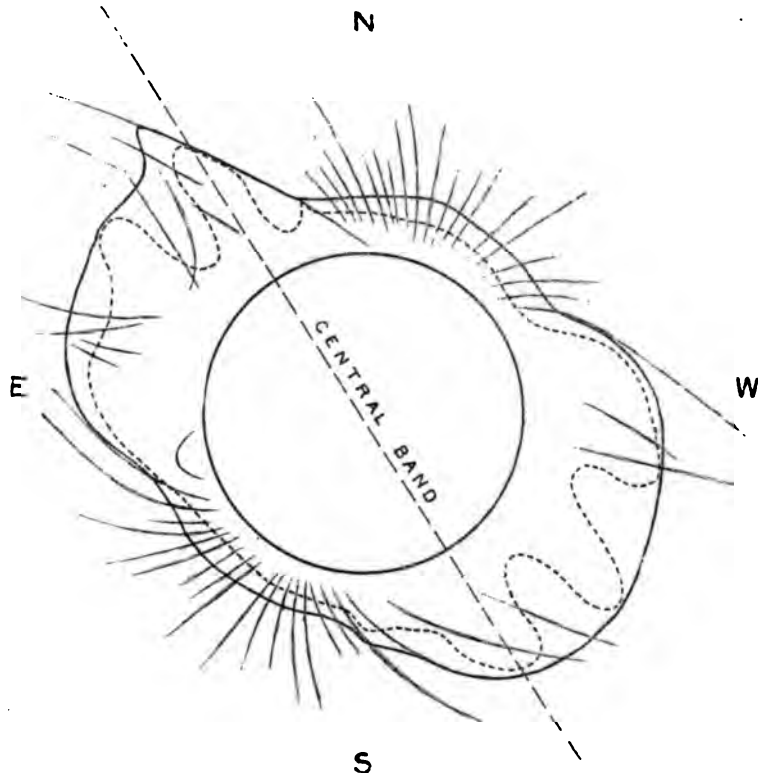
§ 14. *Atmospheric Polarisation.*

The Savart polariscopes A, B, and C were fixed on the framework of *the hut* in such position that the sky in the neighbourhood of the

eclipsed sun could be seen through them. Each was mounted in a turning tube, provided with a pointer and graduated circle and attached to a wooden stand.

The programme of observations was that I should look through A as soon after the beginning of totality as the adjustment and exposures of the 4-prism spectroscope would set me free. The polariscope was to be then turned in its tube until the Savart bands were invisible, and

FIG. 1.



I was at once to set the large photographic Savart camera to the corresponding reading, and to call out to Mr. Wallace, who was making the exposures for that camera.

Next the polariscope B was to be used half-way through totality, when my visual observations with the grating-spectroscope were completed. Finally the polariscope C was to be used towards the end of totality, just before the fifth minute was completed. Each of the polariscopes was to be left as adjusted "for extinction," and the readings were to be made at leisure after the eclipse, except in the case of A, of which the readings were needed at once.

Result.—In the event I used A about 40 seconds after the beginning of totality, and found no trace of polarisation in any position of the instrument. To make sure that nothing was wrong I used also polariscope B, and corroborated the first observation.

Towards the end of the third minute—2^m 50^s from the beginning of totality—I again observed with B. At the end of the fifth minute I used C; and in neither case could I detect a trace of atmospheric polarisation. About 3.50 P.M., after the eclipse was over, I used the polariscopes again, and found them all as adjusted in the morning before the eclipse; each one showed strong bands on the patches of blue sky visible in the field of view.

§ 15. *The Quartz Spectrograph.*

It was intended to attempt to get—

- (i) Photographs of the ultra-violet spectrum of the flash at the beginning and end of totality.
- (ii) A photograph of the ultra-violet spectrum of the corona.

The instrument was fixed on the top of the double tube, and was supplied by light from the coelostat. The arrangement was such that the slit could be set, without readjustment of the image, to catch the crescents both at beginning and end of totality, and the corona during totality. No other medium but quartz was used in the lenses and prisms, and the dimensions were such that a 2-inch beam was transmitted. The image lens had a focal length of 20 inches, and the collimator and camera lenses were each of focal length 18 inches; the prism was compounded of two 30° prisms, one of right-handed and the other of left-handed crystal; all the optical parts were constructed by Hilger. Unfortunately it was found impossible to get the adjustments completed before the eclipse.

On the morning of the eclipse a final attempt was made, and it was thought well to make the exposures according to programme. These were all duly made, but the adjustment was so imperfect that the photographs are useless, which is all the more regrettable as the plates show strong spectra.

§ 16. *Photographic Objective-grating Camera.*

It was intended to attempt to get—

- (i) Photographs of the “flash” spectrum with high dispersion in a limited range of spectrum, to assist in the discussion of the origin of lines, whose wave-lengths it was hoped would be determined accurately by means of the 4-prism spectroscope.
- (ii) A photograph of the blue coronal ring near λ 4231, for comparison with the visual observations of the green coronal ring.

The instrument employed was in essentials that used at Algiers in 1900, but it was mounted differently.

A plane grating by Rowland, 14,438 lines to the inch on a ruled surface $5 \times 3\frac{1}{2}$ inches, was fitted on an axis in front of a telescope of aperture 4 inches and focal length 66.8 inches. The grating is a very brilliant one; the objective is an excellent one, corrected for photographic rays, by Cooke and Sons; and both belong to the splendid spectroscopic installation arranged by the late Professor Piazzi Smyth. I am under great obligation to the Royal Society for the continued loan of this outfit.

The telescope with grating attached was fixed inclined at an angle of 20° to the horizontal and pointing downwards on the sloping pier which supported the double camera, the telescope and camera being parallel. Part of the beam reflected by the *cœlost*at was allowed to fall on a 6-inch mirror, and thereby reflected downwards on to the grating. By inclining the plane of this reflection by the proper amount with the vertical, it was possible to get the crescent at second contact symmetrically disposed with respect to the plane of dispersion of the grating. It was thought best to run the risk involved in the loss of light for the sake of getting the telescope into the very convenient position of parallelism with the double camera, and on the same sloping pier with it.

The light reflected by the small mirror fell upon the grating with an incidence of 39° , and the diffracted beam left the grating at an angle of about 5° on the other side of the normal. In this position H_γ in the spectrum of the second order was not far from the centre of the field of view of the telescope.

The focussing and final adjustment of the spectrum on the focussing plate was intentionally left to be done a few minutes before totality, when the width of the diminishing crescent would make it possible to recognise the exact part of the spectrum in the field of view. Ten minutes before totality the cloud was so obscuring that on looking into the eye-piece I could not distinguish between the spectrum of the crescent and light reflected from the clouds. Returning to the instrument 6 minutes later I found the lines visible, and Lieutenant Briggs, who was to take charge of the exposures with this instrument, turned the tilting screw of the grating till I could see that the dark crescent-shaped lines near λ 4231 were in the centre of the field. I then focussed very carefully, and handed over the instrument to Lieutenant Briggs.

Programme.—Exposures were to be made—

(i) For 1 second when the signal "Stand by" was called by Dr. Wallace 15 seconds before totality.

(ii) For 2 seconds when the count "Thirteen" was called before totality.

(iii) For $4\frac{1}{2}$ minutes, starting at the count "Thirty" after the beginning of totality and ending at the count "Sixty,—five minutes gone."

(iv) For 2 seconds, when I gave the signal "Now" at the end of totality.

Lieutenant Briggs carried out the programme with admirable precision.

Result.—All four photographs are of value. The first serves for determination of the scale of the spectrum and shows that the focussing was very successful. The second and fourth photographs show both bright and dark lines, as was intended; the second is a weak negative, but is full of interesting details; the fourth is quite strong, teeming with bright crescents, of which more than ninety have been counted between λ 4308 and λ 4405. Many of the crescents are bright throughout their whole length; many are reversed in the middle; some of the bright crescents are accompanied by dark crescents on either side; others have a dark crescent only on one side or the other; all varieties seem to be represented.

The third photograph is not very strong but it shows marked continuous spectrum, with the blue coronal ring distinctly though not easily visible. The edges are ill-defined, in marked contrast with the sharp prominence-tips that serve to mark out the positions of the H_γ and H_β chromospheric rings.

The radial extension of the blue coronal ring is about 3', but there is no sign of radial structure.

Two determinations of the wave-length of the light forming the blue ring give the values 4231.4 and 4231.9, if the wave-lengths ascribed to the dark H_γ and H_β lines may be adopted for the bright hydrogen prominences.

The dispersion is about 5.14 tenth-metres to a millimetre, and the scale of the ring is such that its diameter corresponds to 65 tenth-metres. The conditions are thus favourable for the detection of coronal rings. No ring except that at 4231 has been with certainty detected between H_γ and H_β .

§ 17. *Visual Objective-grating Spectroscope.*

An objective grating spectroscope was used as in India, in 1898,* for visual observations of the green coronal ring, with a view to (i) repeating the search made during the Indian eclipse for fine radial streamers, (ii) comparing the distribution of light in the green ring with that in the blue ring near λ 4231, of which I hoped to get a photograph with a photographic objective grating camera.

A plane grating by Rowland, 14,438 lines to the inch, on a ruled

* 'Roy. Soc. Proc.,' vol. 64, p. 57; 'Monthly Notices, Roy. Astro. Soc.,' vol. 58, App., p. [57].

surface $3\frac{1}{2} \times 2\frac{1}{8}$ inches, was fixed on a turn-table in front of a telescope of aperture $3\frac{1}{2}$ inches, and focal length 29 inches. A positive eye-piece with two cross-wires was used, which gave a magnifying power of 19.2, and whose circular field of view was rather more than 1° in diameter.

The instrument was mounted so that the telescope was parallel to the earth's axis and pointed to the south pole. The grating was used in a manner analogous to that in which the mirror of a polar heliostat is used. The light of the corona was incident at an angle of about 57° , and the diffracted beam utilised in the telescope left the grating at an angle of about 13° . In this position of the grating, the green of the second order was used and the magnifying power of the grating was approximately one-half, so that the distorted coronal ring was an ellipse, of which the major axis was about twice as long as the minor axis, the minor axis being parallel to the length of the spectrum and perpendicular to the direction of daily motion. No clockwork was used, but a wedge pushed under a wooden arm fixed to the instrument and projecting radially outwards from the telescope served to give a satisfactory slow motion by slightly turning the whole instrument about what may be described as polar pivots.

The instrument was focussed on the E group of lines about two minutes before totality began. The crescent was small enough to show the lines sharply, though the instrument was so set that the point of second contact was near the end of the major axis of the distorted image.

When I came to make observations of the green coronal ring, I first looked very carefully for fine radial structure; and finding none, I altered the focus very slightly. This did not improve matters, so I reverted to the original focussing, and am quite satisfied about the observation, which could not be more definite.

The observations were begun after the completion of the third minute of totality and were completed in about 80 seconds. Two ellipses had been drawn beforehand to serve as outline for the distorted limb of the moon; and on one of these the record of contour lines of the brightness was quickly but carefully sketched.

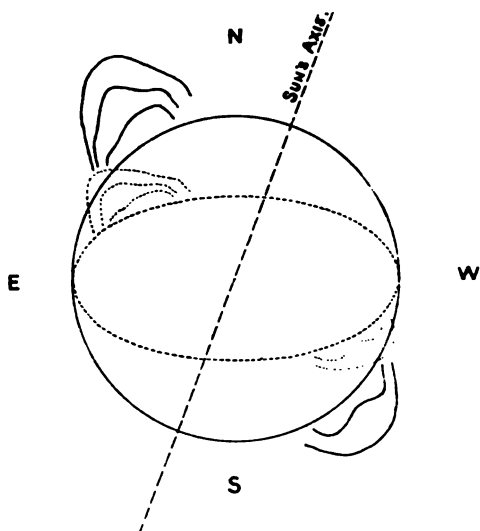
Results.—The coronal ring was distinctly seen, but there was a good deal of other green light throughout the whole of the field of view, doubtless from clouds. No fine radial structure could be seen anywhere round the ring; but there were two broad patches of the light of "coronium" projecting from the elliptical ring.

The accompanying figure (fig. 2) shows by the dotted lines the ellipse drawn beforehand, and the contour lines as sketched in during the eclipse. The unbroken lines indicate the results got by projecting the ellipse into a circle and making the corresponding change in the contour lines.

§ 18. *General Observations.*

The general darkness during totality was greater than that in India, 1898, and in Algiers in 1900, but not so much greater as was anticipated from the great length of duration of totality. No integrating photometer was used. Lamps and candles were provided in case of need, but they might have been dispensed with except for one adjustment.

FIG. 2.



The figure shows, by contour lines of equal brightness, the position of the two bright patches of incandescent "coronium" seen in the corona.

During totality I noted several times an uncoloured ring of light round the moon. It was evidently of terrestrial origin—in fact, a ring such as apart from an eclipse would be described as a "corona." The radius of the ring was estimated as about 1 or $1\frac{1}{2}^\circ$. The equatorial streamers of the true solar corona were estimated as being visible not further than a quarter of a degree from the moon's limb, being then lost in general diffused light. The peculiar feature about the ring, as a phenomenon of diffraction due to the small particles of water forming the cloud to which it was without doubt due, was that the maximum of brightness appeared to be in the ring itself rather than near its centre. After totality colours again appeared in it, but I did not note nor can I recall the diameter of the rings.

"Total Eclipse of the Sun, 1901, May 18. Preliminary Account of the Observations made at Pulo Aoer Gadang, West Coast of Sumatra." By F. W. DYSON, M.A., F.R.S., Sec. R.A.S. Received October 24,—Read at Joint Meeting of the Royal and Royal Astronomical Societies, October 31, 1901.

I. General Arrangements.

The Admiralty having approved of expeditions from the Royal Observatory to observe the Solar Eclipse of 1901, May 18, I was instructed by the Astronomer Royal to occupy a station on the West Coast of Sumatra with the instruments used at Ovar in the eclipse of 1900, May 28. Mr. J. J. Atkinson, who accompanied the Observatory expedition to Ovar, again generously volunteered his assistance and is associated with me in all the observations. His advice and co-operation were of the greatest value throughout.

The programme of observations, which was arranged in concert with the Joint Permanent Eclipse Committee, was essentially the same as that carried out at Ovar.

The 16-inch cœlostat and the 4-inch rapid rectilinear (Abney) lens were lent by the Royal Astronomical Society, and Captain Hills kindly lent the spectroscopic equipment he used at the Indian eclipse, viz., a two prism flint spectroscope, a four prism quartz spectroscope, and a 12-inch heliostat.

At the request of the Royal Society the Admiralty placed H.M. gunboat "Pigmy" at the service of the observers in Sumatra. We are indebted to Lieut. and Com. Oldham for the ready manner in which he assented to our request to anchor off Gadang, and let us live on the "Pigmy." Without his cordial co-operation this station could not have been occupied. We are further indebted to him and to the officers and men of the "Pigmy" for assistance in erecting the huts and instruments, and for the large share they took in the observations on the day of the eclipse.

We received every assistance from the Government of the Dutch East Indies in the choice of station and arrangement with the natives, and were given free passes for ourselves and instruments on the railways. We are specially indebted to Mr. Joeques, the Governor of the West Coast of Sumatra, to Mr. Von Locken, the Assistant Resident at Painan, to Major Müller, the Director of the Ordnance Survey and Chief of the Dutch eclipse party, and to Mr. Delprât, the Director of Railways.

Itinerary.—The observing huts and instruments having been despatched some days previously, the 16-inch cœlostat mirror and some of the photographic plates were taken to Southampton and put

on board the Dutch mail steamer "Koningen Regentes," at Southampton, on March 4. The observers joined the ship at Genoa, on March 14, and sailed in company with Mr. and Mrs. Newall, the Dutch eclipse party, and an American eclipse party from the Massachusetts Institute of Technology, direct to Padang in Sumatra, arriving there on April 6.

In consultation with Mr. Joeke the Governor of the West Coast and Major Müller the Director of the Ordnance Survey as to choice of station, it appeared that the small island of Pulo Aoer Gadang might be suitable, and the Governor kindly offered us the use of his yacht to visit the island on March 10. In company with Mr. Von Locken we chose a suitable site and made the necessary arrangements with the native chief who owned the island. Lieut. and Com. Oldham conveyed us with our instruments and materials for building huts to Gadang on March 13, and remained anchored off Gadang, except for short necessary visits to Padang for ice, fresh meat, &c., till May 25, one week after the eclipse. On this day the instruments were brought back to Padang for shipment to Europe, and the observers were conveyed to Singapore, which was reached in the early morning of June 2.

Station.—The particularly uncertain character of the weather in Sumatra made it desirable that stations as far apart as possible should be occupied by the different parties of observers. The stations occupied by different expeditions are shown on the accompanying map (A).

Sawah Loento, by Mr. Newall and by a party from the Massachusetts Institute of Technology.

Solok, by numerous American observers of the United States Naval Observatory expedition.

Fort de Koch, by American and Dutch observers.

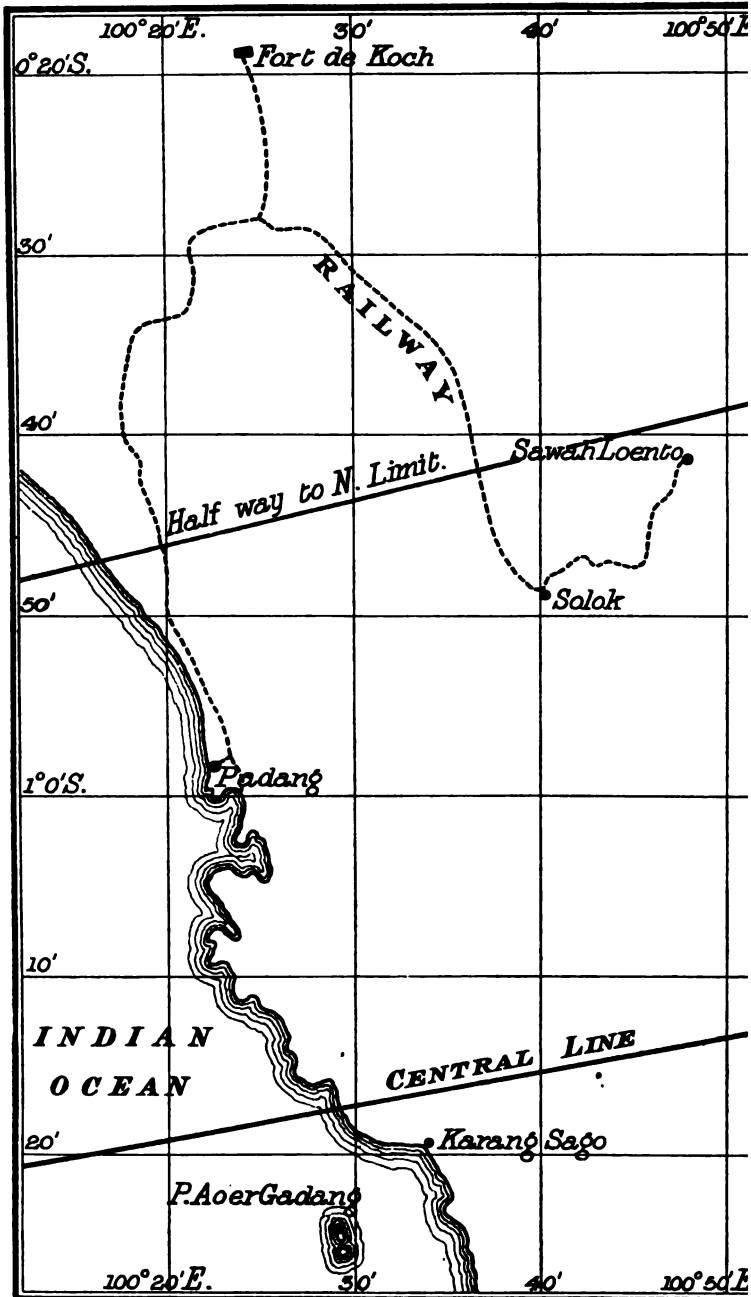
Padang, by the Lick Observatory party, French, Russian, and Japanese observers.

Karang Sago, by the Dutch party.

Aoer Gadang, by the Royal Observatory.

The presence of the "Pigmy" made it possible to occupy Aoer Gadang. This station had the advantage of being as far away as possible from the mountain range which runs parallel to the west coast of Sumatra. Solok and Sawah Loento are to the east of this range.

Aoer Gadang is a small island about 1 mile long and $\frac{1}{4}$ mile broad, surrounded by a coral reef. The site chosen for the eclipse camp was a small clearing near the eastern shore opposite a passage through the reef. Its position, as determined from large scale maps published by the Government of the Dutch East Indies, is lat. $1^{\circ} 23' 28''$ S., and long. $100^{\circ} 29' 13''$ E. of Greenwich. It is about 7 miles distant from



the central line, 6 miles distant from the camp of the Dutch observers at Karang Sago, and 30 miles from Padang.

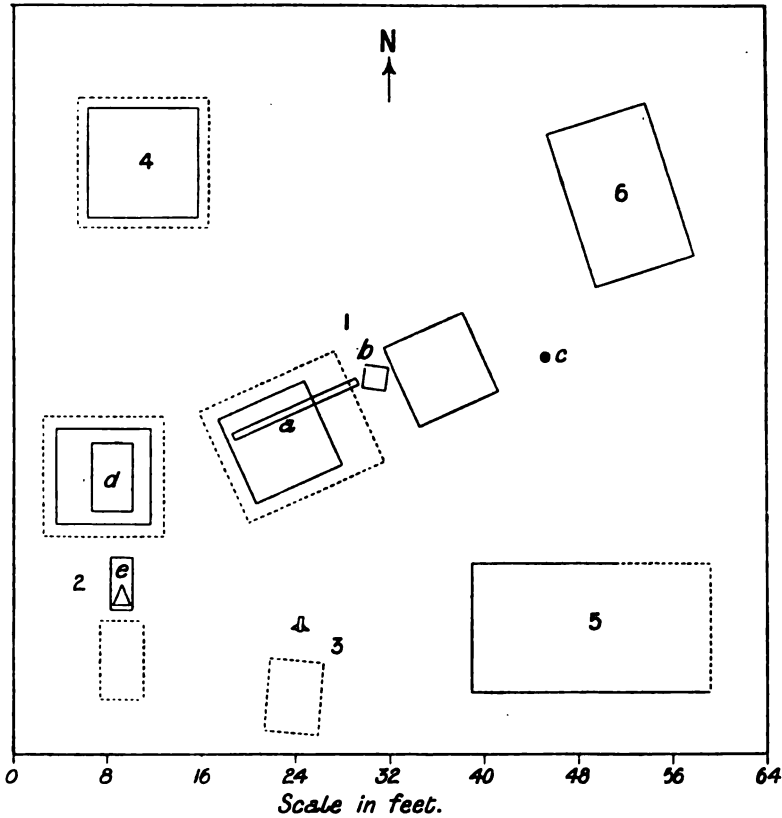
Erection of the Huts and Instruments.—The same light wooden frames covered with Willesden waterproof canvas which were used at Ovar in the eclipse of 1900, May 28, were taken out to Sumatra, with an additional one to serve as a developing room.* As a protection against sun and rain, these were supplemented by sheds, built in Malay style, of bamboos and thatched with *atap*, a species of palm leaf, which is obtained tied on laths about 4 feet long.

One open shed, 14 feet long and 12 feet broad, and 12 feet high at the gable, covered one of the two canvas huts and half of the second one, in which the Thompson coronagraph and the 16-inch cœlostat were placed. As at Ovar, the hut over the cœlostat was moved back a few feet during observations. Thatch was tacked on the roof of this hut. Similar sheds were fixed over the spectroscope hut and over the developing room. Thatch was also tacked all over the developing room hut. This, in addition to black waterproof paper inside, served to make the room light-tight. Movable sheds, each resting on four stout bamboos, were placed over the heliostat and condensing lenses of the spectroscopes and over the Dallmeyer photoheliograph. A house of bamboo and *atap* was also built, open on the east side and partly on the north, in which hammocks were swung, and where the observers lived at times when the "Pigmy" was obliged to leave for a short time. A tent was also erected for the two marines who were left on shore with the observers by Lieut. and Com. Oldham on such occasions. The general arrangement of the station is shown in the accompanying diagram. These huts, which were very satisfactory, were built under the superintendence of two Malay employés of Mr. Delprat, the Director of Railways.

The cœlostat was mounted on a concrete pier sunk about 18 inches into the ground, and rising 4 inches above it. The clock was screwed on to a box which was partly let into the ground, the weight being carried over a pulley on an iron bracket fixed on a palm-tree at a height of about 12 feet. The Thompson tube was mounted on boxes. The table of the spectroscope was placed on four brick piers 4 feet high. The heliostat and condensing lenses were placed on a heavy box which rested on two rows of bricks, which raised it about 4 inches above the ground, the bricks being fixed on a bed of concrete. A foundation of brick and concrete was also provided for the Dallmeyer photoheliograph. In the building of the foundations, erection of piers, and putting together of the canvas huts we received every assistance from the officers and men of the "Pigmy," and are specially indebted to Lieut. Briggs and the engineer, Mr. Townsend. The erection of the huts and instruments occupied us till the beginning of May.

* 'Roy. Soc. Proc.,' vol. 67, p. 393.

PLAN OF ECLIPSE STATION AT AOER GADANG, 1901, MAY 18.



1. { *a.* Thompson coronagraph.
b. Cœlostæt.
c. Palm-tree to which clock-weight was attached.
2. { *d.* Spectroscopes.
e. Heliostat.
3. Double photographic camera.
4. Developing room.
5. Observer's house.
6. Men's tent.

Personnel.—The following list gives the names of those who took part in the observations:—

F. W. Dyson—Quartz spectroscope, Thompson coronagraph.

J. J. Atkinson—Flint spectroscope.

Lieut. and Com. Oldham—Double camera for small-scale photographs. Observed duration.

Lieut. Frewen—Gave signals of beginning and end of totality. Observation of stars.

Engineer Townsend—Set heliostat for corona spectrum, and again for second contact.

Surgeon Nimmo—Exposed at object glass of Thompson coronagraph. Observed duration.

Seven men from the "Pigmy" also assisted; two counting seconds with the aid of a metronome; one exposing in front of the condensing lenses for the spectroscope; one exposing in front of the object glasses of the double camera; and three assisting in handing plate holders to Mr. Dyson and Captain Oldham.

The mode of procedure, which was carefully rehearsed on several occasions previously, was as follows: The observers were stationed at their instruments, and Lieut. Frewen watched the diminishing length of the crescent of the sun on the ground glass of the coronagraph. He had a paper scale on which the lengths of the crescent were marked, computed at intervals of 3^m, 2^m, 1^m, 30', 20', 10' before totality. At 3^m before totality Mr. Frewen called out "Stand by," and the metronome, which had been carefully rated, was started. At 1^m before totality he called out "Get ready." At the words "Thirty seconds" Mr. Dyson and Mr. Atkinson drew out the dark slides ready for exposure. At the word "Ten" the exposures for the "flash" spectrum were begun. At the word "Now" given at the moment of totality the time was recorded by Lieut. Oldham and Dr. Nimmo. Six seconds after totality Mr. Dyson and Mr. Atkinson changed the plates of the spectra and exposed for the corona spectrum, Mr. Townsend moving the image on the slit by the heliostat. Mr. Dyson then went to the Thompson coronagraph and made exposures as detailed below, while Lieut. Oldham exposed with the double camera. When the exposures with the Thompson were finished, about 30' before the end of totality, Mr. Dyson returned to the spectroscope, where he and Mr. Atkinson changed the plates ready for the second flash, and Lieut. Frewen returned to the ground glass of the coronagraph, calling out "Now" at the instant of re-appearance, the times being noted as before by Lieut. and Com. Oldham, and by Surgeon Nimmo.

The Day of the Eclipse.—From May 1 to May 13 the weather was very fine in the early mornings, but the sky became overcast later in the day. On some of these days the observing conditions at the time of the eclipse were excellent, and on all were moderately good. On May 13, however, there was a heavy thunderstorm, and till May 17 the sky was generally overcast about noon. May 17 was a very wet day, and on May 18, the day of the eclipse, it was completely overcast at 6^h. At 8^h it began to clear, but during totality there was a good deal of light cloud in front of the sun. At 1 o'clock the sky was perfectly clear.

The following readings of the dry-bulb thermometer were taken on the day of the eclipse:—

	Local time.			Reading of thermometer.
	hrs.	mins.	secs.	°
Beginning of eclipse	10	48	0	—
	11	0	0	88·8
	11	17	0	86·0
	11	30	0	85·0
	11	45	0	84·3
	12	0	0	81·8
	12	20	0	79·0
	12	19	30	—
Beginning of totality	12	19	30	—
End of totality	12	26	50	—
	13	25	0	84·9
	13	40	0	84·9
	13	53	0	88·0
End of eclipse	13	58	0	—

As the temperature usually rose from 90° to 92°, there would seem to have been a fall of temperature of from 10° to 13° due to the eclipse.

The duration of the eclipse was observed by Captain Oldham as 6^m 20^s·5, and by Dr. Nimmo as 6^m 21^s·5. These times agree with those obtained by the Dutch observers at Karang Sago, who give for the duration 6^m 22^s·* These times are 10^s less than that given in the Nautical Almanac, but agree closely with a prediction kindly forwarded by Dr. Downing (based on a value of 15' 31"·47 for the mean semi-diameter of the Moon, derived from a recent discussion of eclipse observations), viz., 6^m 20^s.

At the commencement of totality at Aoer Gadang a signal was flashed across to the Dutch eclipse camp by the "Pigmy" search-light, thus giving them a warning 16^s before totality occurred at their station.

During totality the sky was much darker than at Ovar in the eclipse of 1900, May 28. Venus, Mercury, and Aldebaran were seen. The corona appeared to be white; the polar plumes and the prominences were distinctly seen.

Development of the Photographs.—As stated in a previous paragraph, a fully equipped dark room was taken out to Sumatra. It was found before the eclipse that considerable care was needed in the development of photographs owing to the high temperature. Immediately after the eclipse the "Pigmy" went to Padang, returning the next day with 250 lbs. of ice. The photographs were developed on May 19 and five following nights. Thanks to the untiring assistance of Mr. Curtain, gunner of the "Pigmy," the development of the thirty-seven

* 'Proceedings of the Koninklijke Akademie van Wetenschappen te Amsterdam, 1901,' p. 87.

photographs, which was a somewhat trying and arduous matter, was carried out very satisfactorily, only one failure of any consequence occurring.

II. *Photographs of the Corona.*

The programme of observations was composed of two distinct parts :

(1.) Photographs of the corona on a large scale to show structural detail.

(2.) Photographs on a smaller scale with rapid lenses, to show the coronal streamers with the greatest possible extension, and to photograph the sky round the Sun for the detection of an intra-Mercurial planet, should one exist.

(1.) *Large-scale Photographs.*—The instrument used was the Thompson photographic telescope with object glass of 9 inches aperture and 8 feet 6 inches focal length, belonging to the Royal Observatory, in combination with a concave telephoto lens by Dallmeyer, of 4 inches aperture and 16 inches focus, fitted as a secondary magnifier, to give an image of the sun 4 inches in diameter, with a field (for full pencils) of 14 inches. The total length of the coronagraph was 12 feet, the equivalent focal length being about 36 feet. The focus was determined by the method used by the Astronomer Royal at the eclipses of 1896, 1898, and 1900, by means of the image of a gauze net in the plane of the plate reflected from the plane mirror of the cœlostat.* In the determination of the focus, which was done at night, Mr. Curtain, warrant officer, rendered great assistance. A cœlostat, with 16-inch plane mirror (made by Dr. Common), was employed to reflect the rays into the coronagraph, which pointed downwards to the mirror at an angle of 3° or 4°, and was in the azimuth 18° north of east on the day of the eclipse. The adjustment of the cœlostat was readily made by observations of the Sun with the attached theodolite in the usual manner.

The camera was furnished with eight plate holders, to take 15 × 15 inch plates, or for the shorter exposures 12 × 10 inch plates in a carrier.

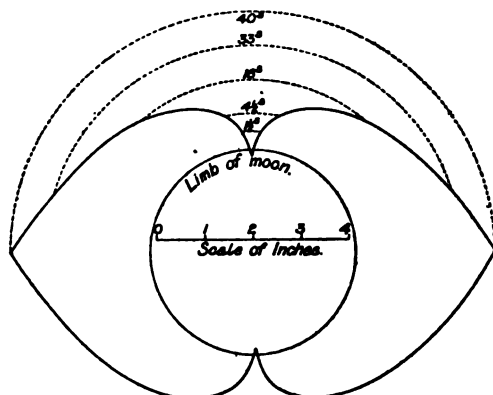
Two of the photographs were taken with a Burckhalter apparatus, made at the Royal Observatory, for graduating the length of exposure at different distances from the Sun's limb. For this purpose a zinc screen, whose form is shown in the diagram, and which was arranged to give what appeared to be suitable exposures at different distances (as shown by Professor Burckhalter's previous experience and Professor Turner's investigation of the diminution of the intensity of the coronal light) was rotated in front of the plate.

To accomplish this, photographic plates were obtained with a hole

* 'Monthly Notices, R.A.S.,' vol. 57, p. 105; 'Roy. Soc. Proc.,' vol. 64, p. 8; vol. 67, p. 397.

in the centre $\frac{3}{4}$ inch in diameter, through which a spindle, mounted on the back of the plate carrier, was passed. On this spindle the screen was fixed in the dark room after the plate had been put in the carrier.

Occluding Shutter for Photographs of Corona by Burekhalter's Method.



The figures against the dotted lines show the effective exposures at different distances from the Sun's centre.

During the exposure, the observer turned a small handle about once a second, which, by a suitable train of wheels, caused the screen to turn about eight times a second. The apparatus appears to work perfectly smoothly, as the limb of the Moon is sharply defined on the photographs taken.

The eight slides were exposed as below, the exposures being made at the object glass by Dr. Nimmo :

	Exposure.			Plate.	
	Begin- ning.	End.	Dura- tion.		
	m. s.	m. s.	secs.		in.
1	0 30	0 40	10	Mawson Photo-mechanical.	12 × 10
2	1 0	1 10	10	Edwards' Ordinary.	12 × 10
3	1 20	1 40	20	Ilford Empress.	15 × 15
4	2 0	2 40	40	" "	15 × 15
5	2 50	3 30	40	Barnet Rocket.	15 × 15
6	3 40	3 45	5	Imperial Ordinary.	12 × 10
7	4 5	4 15	10	" Sovereign	15 × 15
8	4 35	4 55	20	Wratten and Wainwright Ordinary.	12 × 10

A ninth plate was taken at 1 o'clock, local time, on which two exposures were made (the driving clock being stopped for two minutes between) for determination of the orientation of the plates.

"Abney" squares were printed on No. 6 when the plates were put in their carriers the night before the eclipse. The exposure given was 15" to the light of a standard candle at the distance of 5 feet.

Of the eight photographs taken, the two by Burckhalter's method of an occulting shutter, are failures. The failure is not due to any fault in the method, but apparently is entirely owing to insufficient exposure, the corona photographed on the plates not extending as far as had been anticipated from previous eclipses. It seems probable that with a clearer sky these would have been quite satisfactory.

The other six photographs are excellent. The programme of exposures, which was carefully arranged with the Astronomer Royal before leaving England, has resulted in a well graduated series of photographs from No. 1, which shows the prominences and a small amount of corona to No. 5 in which the extension is shown to about 25' from the Moon's limb. The polar plumes and the coronal streamers are well brought out, as well as the remarkable detail of the inner corona. Two features call for special note: (1) the polar streamers round the Sun's north pole are not nearly so extensive as those round its south pole; (2) a very remarkable arch in the corona round the large prominence at position angle 145° (measuring N.E.S.W.). Round this prominence three separate arches are shown, one inside the other, their radii being 1'·2, 2'·4, and 3'·7 respectively. They have the appearance of layers of clouds over an eruption.

(2.) *Small-scale Photographs to show Extension of the Corona.*—The double camera was used in this eclipse exactly as at Ovar to carry a Dallmeyer rapid rectilinear lens of 4 inches aperture and 34 inches focus working at $f/8$ (lent by the Royal Astronomical Society), and a "Unar" lens, by Ross, of 2·4 inches aperture and 12 inches focus, working at $f/5$.

Eight plate-holders, each taking a pair of 16 cm. × 16 cm. plates, were used during totality. The exposures, to avoid any possibility of shake, were made by holding a cover in front of the object glasses.

The mounting was exactly as at Ovar.

The exposures were as follows:—

No.	Duration. secs.	Plate.
1	5	Wratten and Wainwright Instantaneous.
2	20	Ilford Empress.
3	10	Imperial Ordinary.
4	35	Imperial Special Rapid.
5	20	Imperial Ordinary.
6	35	Imperial Sovereign.
7	5	Wratten and Wainwright Instantaneous.
8	10	Imperial Ordinary.

Abney squares were printed on No. 6 by exposure to a standard candle at a distance of 5 feet for 4 minutes with the Dallmeyer lens, and 8 minutes with the Unar lens. It had been intended to print squares on several plates, but the number of photographs to be dealt with made it necessary to limit work as much as possible the night before the eclipse when the photographic plates were taken from sealed tins and put in their carriers, and after the eclipse it was necessary to finish the developing as soon as possible. Of these photographs Nos. 5, 6, 7, 8 are the most successful. The extension shown with the Abney lens reaches to 66' from the centre, and with the Unar lens to 77'.

Venus and Mercury and a number of stars are shown on the Unar plates. No. 5 is the best, and shows 7 stars of the Pleiades and 25 others of magnitudes ranging from 4 to 6, which have been identified. Unfortunately, owing to the clouds, stars are not equally well shown all over the plate, and δ Tauri ($4^m.0$) and θ Tauri ($3^m.9$) are not shown; γ Tauri ($3^m.9$), which is near θ Tauri, is, however, shown.

It may be concluded that any object appreciably brighter than $4^m.0$ within the area covered by the plate (*i.e.* within 15° of the Sun) would have been shown.

III. *The Spectroscopic Cameras.*

The two spectroscopes used by Captain Hills at the Indian Eclipse of 1898 were kindly lent by him, and were used in Sumatra with the following adjustments:—

	Spectroscope No. 1.	Spectroscope No. 2.
Objective	Cooke, achromatic, 4½-in. aperture, 6-ft. 2½-in. focus.	Single quartz lens, 5-in. aperture, 4-ft. 7-in. focus.
Collimator and camera lenses	Single quartz lens, 2½-in. aperture, 30-in. focus.	Single quartz lens, 8-in. aperture, 33½-in. focus.
Prisms	Two dense flint prisms of 60°, 4½-in. base, 2½-in. height.	Four double quartz prisms of 60° (each prism being composed of two half-prisms of right- and left-handed quartz), 3½-in. base, 2½-in. height.
Slit.	1½ in. by 0.0015 in.	2 in. by 0.0012 in.
Prisms at maximum deviation for	H (λ 4340)	λ 3500.

The width of the slit of the flint spectroscope was obtained by means of the diffraction images, and that of the quartz was checked in the same manner.

The length of the spectrum of the flint is 3½ inches from H_β

(λ 4861) to K (λ 3934), and for the quartz 3 inches from λ 4000 to λ 3300. In order to obtain a greater length of spectrum in focus, the plate holders were made, at Mr. Davidson's suggestion, to carry two plates $3\frac{1}{2}$ inches wide, inclined at a suitable angle, instead of one $6\frac{1}{2}$ inches wide.

Both spectroscopes were mounted horizontally, and supplied with light by a heliostat furnished with a 12-inch flat mirror. The heliostat was adjusted in the usual manner by means of the attached theodolite.

Programme of Exposures.—The two spectroscopes were adjusted to be as nearly as possible on the Sun's limb simultaneously, and the programme of exposures was the same for both. The cameras were provided with rack movements so that a number of exposures could be made on the same plate. The programme arranged to take—

(i.) A number of short exposures from 10^s before to 6^s after the beginning of totality for the "flash" spectrum.

The plate carriers were then changed, and

(ii.) An exposure for the spectrum of the corona made from 15^m or 20^s after the beginning of totality, and continued for $5\frac{1}{2}$ minutes—the exposure ending at $5^m 50^s$ from the beginning of totality.

The plate carriers were again changed, and

(iii.) From $6^m 10^s$ from the beginning or 10^s from the end of totality, exposures were to be made for the "flash" at third contact.

Between (i) and (ii) the image of the Sun was to be brought by the slow motion of the heliostat from being nearly tangential to the slit to a position in which the horizontal diameter was divided by the slit in a ratio of 1 : 3. Again, between (ii) and (iii) it was to be brought nearly tangential.

A hitch occurred between (ii) and (iii), and the "flash" at second contact was not obtained.

The plates used for the "flash" at the beginning of totality were Edwards' Isochromatic and Ilford Empress for the flint spectroscope, and Ilford Empress for the quartz.

For the corona, "Snap-shot," Isochromatic, and Rocket plates were used for the flint spectroscope, and Imperial Special Rapid for the quartz.

The Corona Spectrum.—The continuous spectrum of the corona is shown from D_3 (λ 5876) to λ 3350. The green line at 5304 is strongly shown. The lines at 4568, 4233 and 3987 are shown. No new lines are shown. In the ultra-violet the lines at 3801, 3456 and 3381 are strongly shown, and possibly some fainter lines. The wave-lengths have not yet been determined accurately. No absorption lines are shown.

The Chromospheric Spectrum.—This is obtained on the different plates extending from D_3 (λ 5876) to a little beyond 3300. The photographs are not on the whole so good as those obtained at Ovar, but will serve

to supplement at the two ends of the spectrum, 70 lines being shown between $H\beta$ ($\lambda 4861$) and D_3 ($\lambda 5876$), and 100 between 3500 and 3296.

“Total Eclipse of the Sun, 1901, May 18. Preliminary Account of the Observations made at the Royal Alfred Observatory, Pamplemousses, Mauritius.” By E. WALTER MAUNDER, F.R.A.S. Received October 24,—Read at Joint Meeting of the Royal and Royal Astronomical Societies, October 31, 1901.

General Arrangements.

An expedition from the Royal Observatory to observe the Solar Eclipse of 1901 in the island of Mauritius having been sanctioned by the Admiralty, I was instructed by the Astronomer Royal to proceed to that island, there to act in concert with the Director of the Royal Alfred Observatory, Mr. T. F. Claxton, who had expressed his desire to co-operate in the observation of the eclipse. In accordance with a scheme approved by the Joint Permanent Eclipse Committee of the Royal and Royal Astronomical Societies, I took out with me two instruments belonging to the Royal Observatory, Greenwich, for photographing the corona; the one giving an image of the Moon 2·4 inches in diameter, and intended to secure the general structure of the corona, and the other giving an image 0·3 inch in diameter, and intended to secure the outer coronal streamers. The Joint Permanent Eclipse Committee lent for use in combination with these instruments two coelostats, the one carrying a mirror of 16 inches diameter, the other one of 12 inches. A third mirror, one of 12 inches diameter, was kindly lent by Mr. Frank McClean, F.R.S. My equipment was completed by the generosity of Mr. John Evershed, F.R.A.S., who very kindly placed his prismatic camera of 2 inches aperture at the disposal of the Astronomer Royal for my use in the eclipse.

I was warmly welcomed in Mauritius by the Director of the Royal Alfred Observatory, Mr. T. F. Claxton, and by his Chief Assistant, Mr. A. Walter. Both identified themselves completely with me in my work, and helped me in every possible way. We therefore combined the instruments which I had brought out with those of the Observatory, and worked with them as though we formed but one party, and as if the instruments were all part of the same equipment.

I was also greatly indebted to Sir Charles Bruce, G.C.M.G., the Governor of the island, and to Sir Graham Bower, K.C.M.G., the officer administering the Government during the Governor's absence, for the ready hospitality which they extended to me. Through their action, also, my instruments were landed without Customs examina-

tion and were conveyed by rail, free of cost, up to Pamplemousses, and back again after the eclipse was over. Free passes on all the railways of the island were also issued to me, and to Mrs. Maunder, who had accompanied me with a special instrumental equipment of her own. The military authorities also rendered great assistance. Lieutenant-Colonel H. J. Lyster, R.G.A., gave permission to nine non-commissioned officers of the garrison, who had volunteered for the purpose, to come down to assist in the eclipse observations. Of these, five also helped in the unpacking and erection of the instruments, and one, Staff-Sergeant R. M. Smith, A.O.C., gave great assistance in the cleaning, repair, and rating of the several driving-clocks. The expedition was also indebted to the Hon. Hamilton Stein, who undertook the re-shipping of the instruments for the homeward voyage; to Mr. G. Ireland, who supplied tarpaulins for covering the skeleton huts erected to shelter the instruments; to Captain A. W. de Wilton, Inspector-General of Police, who sent constables to keep the Observatory grounds from intrusion on the morning of the eclipse, and to Mr. D. P. Garrioch, Superintendent of Inland Revenue, who arranged for the greater part of the provisioning of the eclipse party during their stay at the Observatory.

Itinerary.—The instruments for the Mauritius expedition were despatched from the Royal Observatory on March 6, with the exception of the prisms and object-glass of the prismatic camera lent by Mr. John Evershed, F.R.A.S., which I took to Marseilles with my personal baggage. The instruments were embarked at the Royal Albert Docks on the ss. "Bagdad" of the Messageries Maritimes line, and transhipped from that vessel to the ss. "Melbourne" of the same line at Marseilles. I left London on March 23, and proceeded overland through France to Marseilles. Owing to the great dock strike then prevailing at Marseilles, the ss. "Melbourne" was not able to leave until the evening of March 26. The delay was, however, made up for by greater speed on the voyage, and Mauritius was reached on April 20, as had been originally expected. The return voyage was unexpectedly delayed, as the Messageries line quarantined the island on account of the plague there, and the Union-Castle steamers having ceased to run to Mauritius, in consequence of the South African war, the British India line *via* Colombo was the sole remaining homeward route, and even on this line the steamers only ran once a month. It was not possible, therefore, to leave Mauritius again until July 12, when I embarked on the ss. "Warora"; reaching Colombo on July 22. Here I had to wait until July 25, when I left by the ss. "Mombassa," also of the British India line, and reached London August 25. The instruments left Mauritius in the ss. "Slingsby" on July 12, and were received at the Royal Observatory, Greenwich, on September 10.

Station.—The station occupied was the Royal Alfred Observatory,

Pamplemousses, in the low-lying plain which occupies the north of the island of Mauritius. This was about 15 miles to the N.W. of the central line, but as the eclipse was one of long duration, the distance from the central line made but little difference to the length of totality, and several important considerations led to its adoption. First of all, the probabilities of fine weather at the time of the morning when the eclipse would take place, appeared much greater for the low ground in the north of the island, than for the hills in the centre; an anticipation which the event fully justified; for whilst the total phase of the eclipse was observed under favourable conditions at Pamplemousses, it was entirely lost in cloud at Curepipe, on the central line on the high ground in the interior. Next, there were the great advantages offered by the Observatory itself of accurate determination of time, of a site the co-ordinates of which were known, of two instruments on permanent mountings, an equatorial and a photoheliograph, suitable for use in the eclipse, and of two efficient and well constructed dark-rooms for photography.

The observing station was therefore fixed in the Observatory grounds, in Lat. $20^{\circ} 5' 39''$ S. and Long. $3^{\text{h}} 50^{\text{m}} 12^{\text{s}}.6$ E. according to the "Nautical Almanac," on the authority of the late Dr. C. Meldrum, the former Director.

Erection and Arrangement of the Instruments.

The instruments were landed from the "Melbourne" at Port Louis, on April 23, and were conveyed by rail to Pamplemousses, and from thence by road to the Observatory, the same day. The next day they were unpacked, and the positions which they were to occupy were chosen.

The main building of the Observatory is rectangular in form, and nearly but not exactly orientated. Its front or north side is $76\frac{1}{2}$ feet in length, and its breadth is $58\frac{1}{2}$ feet. Opposite the north front and distant from it 160 feet, is the Magnetic Basement, a structure of which only the roof is above the ground, and which is 43 feet square. A broad lawn extends between the two buildings, and the instruments were erected for the most part at the west end of the lawn, so as to command as much of the eastern sky as possible. It was, however, found necessary to cut down a clump of trees on the eastern side of the lawn to a height of 16 feet in order to secure an uninterrupted view of first contact, which would take place when the sun was only 5° high, and this Mr. Claxton accordingly had done.

The instruments were mounted in two divisions. The first division consisted of the Mauritius photoheliograph, which was dismounted from its equatorial stand in a detached dome in the Observatory grounds, and of a rapid rectilinear lens by Dallmeyer, of 4 inches

aperture and 32 inches focal length. These two instruments were placed horizontally, the camera of the R.R. lens being placed immediately above the photoheliograph tube, and both were supplied with light by means of a 16-inch mirror mounted on a cœlostat. The second division consisted of the Greenwich coronagraph, and Mr. Evershed's prismatic camera. The coronagraph was mounted in a horizontal position, and the prismatic camera was fixed immediately above it, both instruments being supplied with light by a 12-inch mirror mounted on a cœlostat. Between the two a clock was mounted which rang an electric bell close to the camera end of the photoheliograph, and another close to the 12-inch cœlostat, at every tenth second during totality. The instruments were fixed upon boxes filled with stones, and to shelter them skeleton huts were formed, each consisting of three pairs of poles carrying a ridge-pole. Tarpaulins were then thrown over the ridge-pole, and pegged down to the ground. When any instrument needed to be used, one or more of the tarpaulins could be thrown up on the side upon which it was desired to observe. During the eclipse all the tarpaulins were entirely removed, except one fixed round the 10-second clock, which was left in order to shelter the pendulum from the wind.

The positions of the instruments relative to the Observatory buildings are shown in the accompanying plan, in which are also indicated the positions of Mrs. Maunder's instruments, which were also set up in the Observatory grounds.

The above arrangement of the instruments was finally completed by May 11 when the prismatic camera was mounted, one week before the eclipse.

Personnel.—The following list gives the names of those who took part in the observations:—

With the 16-inch cœlostat and its telescopes—

T. F. Claxton—Mauritius photoheliograph. Large-scale photographs of inner corona.

Bombardier A. J. Cox—Recorder to Mr. Claxton.

Bombardier C. A. Truman—Carried plate-holders for Mr. Claxton to and from dark-room during the partial phase.

Charles G. Garrioch—Changed the plates in the plate-holders during the partial phase.

T. A. Pope—Rapid rectilinear lens. Small-scale photographs to show extension.

E. G. Rowden—Exposed at the object-glasses of both the photoheliograph and the R.R. lens for Mr. Claxton and Mr. Pope.

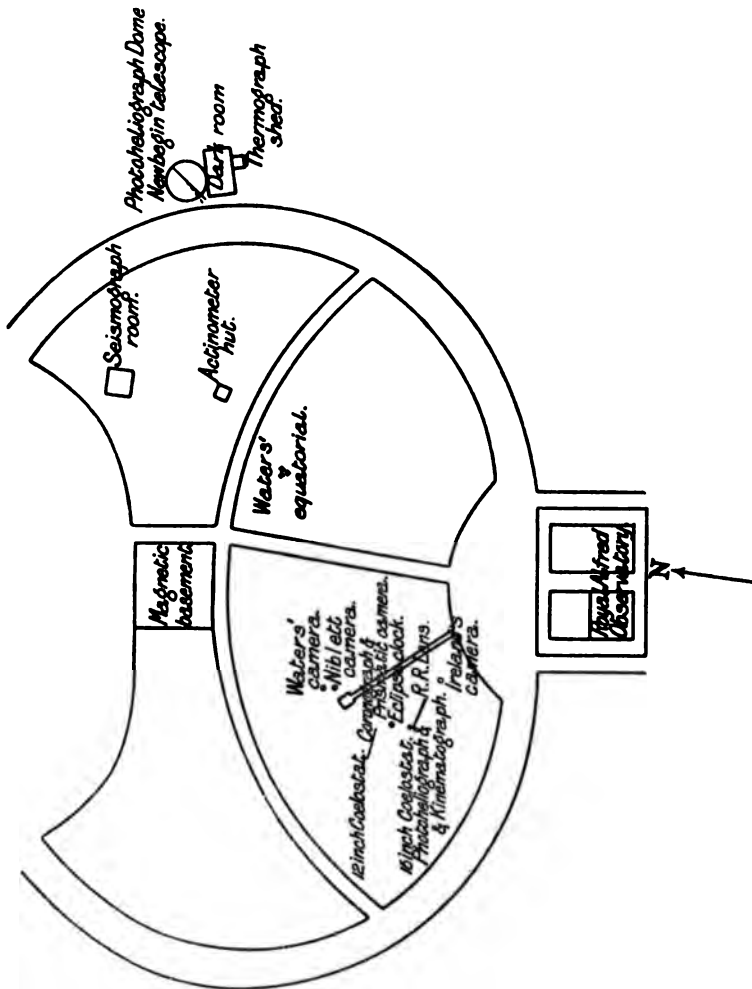
With the 12-inch cœlostat and its telescopes:—

E. Walter Maunder—Greenwich coronagraph. Large scale photographs of corona.

Lance-Corporal Lilley—Recorder to Mr. Maunder.

Bombardier W. Bailey—Moved rack-work of the Evershed prismatic camera.

Colour-Sergeant-Major W. Wade—Exposed at the object-glasses of both the coronagraph and the prismatic camera.



Plan of the Grounds of the Royal Alfred Observatory, Mauritius, showing the distribution of the Instruments during the Total Solar Eclipse of 1901, May 18.

With the 10-seconds eclipse clock :—

Staff-Sergeant R. M. Smith—Time-keeper at 10-second clock.

F. W. Robertson, Lieutenant, R.E.—Recorded times of the 10-second signal-bells.

With the 6-inch equatorial of the Mauritius Observatory :—

F. T. Piggott—Observed with the 6-inch equatorial.

N. V. Olivier—Recorder to Mr. Piggott.

The following was the method of procedure, which was carefully rehearsed many times on May 16 and 17. The observers took their places at their instruments, and Mr. Claxton watched the diminishing arc of the Sun on the ground glass of the photoheliograph, and at 30 seconds before second contact gave the word to "Stand by." At 20 seconds I gave the word to Sergeant-Major Wade to begin the exposure of the plates in the prismatic camera. At 10 seconds, when the arc of sunlight had lessened down to one of 49° , Mr. Claxton called "Ten," the signal to Staff-Sergeant Smith to start the 10-seconds striking eclipse clock, and Staff-Sergeant Smith called the number of the bells as they rang out at every tenth second from this moment until some time after third contact. The exposures at the different cameras were then made at the sound of the bells, and Lieutenant F. W. Robertson, R.E., entered the time of each bell as it rang; a very simple arrangement, which worked smoothly and well, and gave the times of exposure of the different plates very closely.

The Day of the Eclipse.—The weather at eclipse time—i.e., from $6^{\text{h}} 51^{\text{m}}$ to $9^{\text{h}} 5^{\text{m}}$ A.M.—had been by no means promising for the first three weeks after landing, but had tended to improve later. The morning of May 18 was the first upon which the Sun had been entirely free from cloud at the time of totality, $7^{\text{h}} 53^{\text{m}}$ A.M., and even on that morning first contact was lost by the interposition of a dense bank of cloud, which came up from the east soon after sunrise and overtook the Sun. It passed away in a few minutes, and the first photograph of the partial phase was taken at 16 minutes after the predicted time for first contact. Light scud continued to pass over the Sun for about 40 minutes more, but got thinner and lighter as totality drew on, and about $2\frac{1}{2}$ or 3 minutes before second contact the entire eastern half of the sky was free from cloud, and remained so until after fourth contact. But though the sky was thus apparently clear there was evidently much moisture in the air, since at Quatre Bornes, 13 miles to the south-west, the total phase was observed in a smart drizzle of fine rain; and at Curepipe, 16 miles to the south, it was entirely lost by thick cloud. The images also, as seen upon the ground glasses of the photoheliograph and coronagraph, were very unsteady, the Sun's limb "boiling" excessively. This "boiling" effect would seem to have been less noticeable in the 6-inch equatorial mounted in a dome on the roof of the Observatory main building, and in the Newbegin telescope brought by Mrs. Maunder and mounted in the photoheliograph dome of the Observatory, and which were therefore at a considerable elevation, than in the instruments fed by the two

celestals which were necessarily quite close to the ground. The position of the latter, in the open air and placed horizontally, would render them more exposed to the effect of ground currents than the two other telescopes would be, pointing upwards at an angle of 19° and within domes.

The darkness during totality to the observers out in the open air was not great, the general illumination being considerably greater than at the full Moon. Five observers, noting the twilight illumination in the evening after the eclipse, considered it equal to that of mid-totality at the following local times:—

	h.	m.
Mrs. Claxton	5	58 P.M.
Mrs. Maunder	5	57
Mr. Claxton	5	58
Mr. Hall	5	58
Mr. Maunder	5	57

corresponding in the mean to 31 minutes after geometrical sunset, or to a position of the Sun, neglecting refraction, of 6° 53' below the horizon.

But the observers in the equatorial and photoheliograph domes found it very dark during totality. A sudden gust of wind came from the N.E. at the moment of second contact and blew out the observer's candle in the equatorial dome. The recorder here, Mr. Olivier, found it quite impossible to read his chronometer or to write in the darkness, and hence Mr. Piggott's determination of the times of the second and third contacts were wholly lost.

Venus and Mercury were both very brilliant, side by side, some 5° below the Sun, whilst Jupiter and Saturn were prominent in the western sky. No search was made for other celestial objects.

Times of the Contacts.—The following determinations were made of the times of the three last contacts. As already stated, the first contact was lost through cloud:—

Observer	T. F. Claxton. R. Alfred Obs.	E. W. Maunder. R. Alfred Obs.	A. Walter. R. Alfred Obs.	Capt. Roberts ss. "Ugina."
Place	Photoheliograph	Coronagraph.	Newbegin telesc.	Navy telescop
Instrument	4 in.	4 in.	4½ in.	2 in.
Aperture	Kullberg 3400.	H. White 1424.	Webster 925	
Chronometer				
Error of chrono- meter	1 ^m 0 ^s 4 f.	3 ^h 53 ^m 47 ^s 3 s.	2 ^m 58 ^s s.	
Observed times—				
Second contact.	..	3 57 52.2	7 ^h 48 ^m 43 ^s	
Third contact..	..	4 1 25.1	7 52 16	
Fourth contact	9 ^h 5 ^m 43 ^s 0	5 10 55.1		
True local times—				
Second contact.	..	7 51 39.5	7 51 41	7 ^h 51 ^m 38 ^s
Third contact..	..	7 55 12.4	7 55 13	7 55 12
Fourth contact	9 4 42.6	9 4 42.4		

Captain Robertson was not present at the Royal Alfred Observatory, but observed the eclipse from the deck of his ship, ss. "Ugina," of the British India Steam Navigation Company, in Port Louis harbour, $6\frac{1}{2}$ miles S.W. by W. of the Observatory.

An accident prevented Mr. Claxton from observing the second and third contacts. Mr. Claxton and myself watched the contacts on the ground-glass screens of our respective cameras.

Times of the 10-second Bells.—The times of the 10-second bells of the eclipse clock—which was one of the clocks used by the late Sir G. B. Airy, K.C.B., in the Harton Colliery experiment—were recorded by Lieutenant F. W. Robertson, R.E., with the chronometer Kullberg 3400, that chronometer being $1^m 0^s.4$ fast.

No. of Bell.	Observed time.			True local time.		
	h.	m.	s.	h.	m.	s.
1	7	52	42.3	7	51	41.9
2			52.3			51.9
3	53		2.3	52		1.9
4			12.2			11.8
5			22.1			21.7
6			32.1			31.7
7			41.9			41.5
8			51.9			51.5
9	54		1.8	53		1.4
10			11.8			11.4
11			21.8			21.4
12			31.7			31.3
13			41.8			41.4
14			51.8			51.4
15	55		1.8	54		1.4
16			11.8			11.4
17			21.7			21.3
18			31.7			31.3
19			41.7			41.3
20			51.8			51.4
21	56		1.6	55		1.2
22			11.6			11.2
23			21.6			21.2
24			31.6			31.2

Staff-Sergeant R. M. Smith called out the numbers of the bells as each rang.

Photographs of the Corona.—These were taken on three different scales; the first on a scale of 8 inches to the Moon's diameter, to show the prominences and lowest corona; the second on a scale of 2.4 inches, to show the general structure of the corona; the third on a scale of 0.3 inch, to show the coronal streamers to the greatest possible extension. The aperture employed in each case was the same, viz., 4 inches, and the light-gathering powers on the plate of the three instruments were nearly in the proportion of 1, 11, and 700.

(1.) *Photographs on the 8-inch Scale.*

These were taken by Mr. T. F. Claxton with the photoheliograph of the Royal Alfred Observatory, Mauritius, the object-glass of which is 4 inches in aperture and 5 feet in focal length. It gives an image of the Sun nearly 8 inches in diameter, being fitted with a secondary magnifier, enlarging the image in the primary focus $13\frac{1}{2}$ times. The total length of the photoheliograph was $9\frac{1}{2}$ feet; its equivalent focal length 69 feet. The camera was constructed to carry plates 10 inches square, and it was proposed to expose six plates during totality for photographs of the corona, and as many as possible during the partial phase for determination of the Moon's place, and of the relative diameters of the Sun and Moon. Only three plates were exposed during totality, the handle of the shutter of the fourth plate-carrier breaking short off when the shutter was opened. The plate-carrier was thus locked in the camera, and it was not possible to release it until the total phase was over. The three plates taken were exposed as below, the exposures being given by Mr. Rowden at the object-glass of the instrument at the sound of the signal bells.

No.	From	To	Duration of exposure.	Plate.
1	Bell 3	Bell 5	sec. 20	Edwards' Medium.
2	" 6	" 8	20	Imperial Ordinary.
3	" 8	" 11	20	" Special Rapid.

After totality the clock of the cœlostæt was stopped, and three photographs of the Sun were taken on the same plate for orientation. The true local times of the first and last of these were :—

h.	m.	s.
8	15	20·1
8	16	5·2

The time of the second exposure was not recorded. The plate was an "ordinary lantern" by Cadett and Neale. The aperture of the photoheliograph was reduced to $1\frac{1}{2}$ inches, and the exposure was given by the drop-slit in the primary focus.

Two sets of "Abney squares" were printed upon No. 3 on June 13, before the plate was developed. Both sets were printed by the light of a Sugg's Standard candle; the one being exposed for 4 seconds, the other for 64 seconds, the candle being at a distance of 10 feet in both cases.

The corona is seen only on the E. side of the Sun, as the image is

somewhat out of the centre of the plate in all three of the photographs; and owing partly to shake in the instrument, and partly to the excessive atmospheric disturbance, which under the high magnification employed was very apparent, the definition is poor in each case.

No. 1. The corona is shown to a height of about 1 minute of arc. The image is faint.

No. 2. The corona is shown to a height of about 3 minutes of arc. The image is fairly dense.

No. 3. The corona is shown to a height of about 2 minutes of arc. The image is faint and flat.

The Mauritius photoheliograph was focussed by repeated photographs of the Sun, taken during the fortnight preceding the eclipse.

(2.) *Photographs on the 2·4-inch Scale.*

These were taken with the object-glass of the Dallmeyer photo-heliograph No. 5, belonging to the Royal Observatory, Greenwich—and which, like that of the Mauritius photoheliograph, had an aperture of 4 inches and a focal length of 5 feet—used in connection with a concave telephoto lens by Dallmeyer, of 3 inches aperture and 12 inches focal length. The instrument thus constructed has been referred to for convenience as the “Greenwich coronagraph,” and was about $7\frac{1}{2}$ feet in actual length, with an equivalent focal length of nearly 21 feet. It was furnished with ten plate-holders to take 12 by 10-inch plates, and it was proposed to take seven photographs during totality.

These photographs were taken by myself, Colour-Sergeant-Major Wade exposing at the object-glass of the instrument by the signal bells as follows:—

No.	From	To	Duration of exposure.	Plate.
			secs.	
1	Bell 2	Bell 3	10	Wratten and Wainwright Instantaneous.
2	„ 4	„ 6	20	Ilford Empress.
3	„ 7	„ 11	40	Imperial Special Rapid.
4	„ 12	„ 15	30	„ Sovereign.
5	„ 16	„ 18	20	„ Fine Grain Ordinary.
6	„ 19	„ 20	10	„ Special Rapid.
7	„ 21	—	4	„ Ordinary

After the ground glass had been inserted for the observation of third contact, and had been removed again, the aperture was diminished to half an inch, and the clock of the celostat stopped and three photo-

graphs were taken of the Sun, on the same plate for orientation. The plate was an Imperial Fine Grain Ordinary and the exposure was as quick as could possibly be given by uncovering and covering the object-glass by hand. The true local times of the three exposures were :—

	h.	m.	s.
	7	58	39·3
	7	58	57·3
	7	59	17·3

A ninth plate, a Wratten and Wainwright Instantaneous plate, was exposed with the same aperture and exposure at 8^h 0^m 17^s·3. The plate in the tenth plate-holder was not used.

Two sets of "Abney squares" were printed upon No. 4 on June 13, before the plate was developed. Both sets were printed by the light of a Sugg's Standard candle, at 5 feet distance; the one being exposed for 15 seconds, and the other for 4 minutes.

No. 1 is a clean and fairly dense negative, showing well the polar rays and the structure of the lower corona, especially in the east equatorial wing. The west wing shows less detail. The corona is traceable to a distance of 15 or 16 minutes from the limb of the Moon in the S.E. ray.

No. 2 is a clear thin negative showing much about the same detail as No. 1.

No. 3 is a thin negative but shows a considerable extension of the great S.E. and N.E. rays and a good deal of detail in their lower regions.

No. 4 is a dense negative showing the greatest degree of extension in the coronal streamers of any of the series. In the case of the two chief rays, the corona can be traced to a distance of nearly half a degree from the Moon's limb.

No. 5 shows the lower corona well up to 5 or 6 minutes from the Moon's limb.

No. 6 is partly spoiled by fog, and only the lower corona is seen up to about 3 minutes.

No. 7 is a faint and delicate negative showing the chromosphere, prominences, and the lower corona up to a distance of about 3 minutes from the limb of the Moon.

Two methods were employed to focus the Greenwich coronagraph; the first being the method described by the Astronomer Royal in his Reports of the Eclipse Expeditions of 1896, 1898, and 1900,* an image of an object (gauze net in the plane of the plate) being photographed by reflection normally from the plane mirror of the cœlost. The second method was by photographing Arcturus, the image of which was allowed to trail across the plate.

* 'Roy. Soc. Proc.,' vol. 64, p. 8; and 'Monthly Notices, R.A.S.,' vol. 57, p. 106, and vol. 60, p. 397.

(3.) *Photographs on the 0·3-inch Scale.*

These were taken by Professor T. A. Pope, of the Royal College, Mauritius, with a Dallmeyer rapid rectilinear lens of 4 inches aperture, and 32 inches focal length, Mr. E. G. Rowden exposing at the object-glass at the same time as he exposed for the Mauritius photoheliograph, which was mounted immediately below it. Six plate-carriers were provided for this instrument, and six photographs of the corona were secured.

No.	From	To	Duration of exposure.	Plate.
1	Bell 3	Bell 5	sec. 20	Imperial Process.
2	" 6	" 8	20	" Ordinary.
3	" 9	" 11	20	" Special Rapid.
4	" 12	" 14	20	Ilford Empress.
5	" 15	" 17	20	Wratten and Wainwright Instantaneous.
6	" 18	" 20	20	Imperial Fine Grain Ordinary.

The plates used were 16 cm. square, and the Sun was placed a little to the west of the centre of the field, so that Venus and Mercury might be included in the photographs. As they are well shown on all the plates, there was no need for any other mode of orientation, and no special photographs were taken for that purpose. Nor were the "Abney squares" printed on any of the plates with this instrument.

No. 1. A good clear negative. On the west side of the corona four straight rays are seen, the two longest extending about $1\frac{1}{4}^{\circ}$ from the Moon's limb. On the east two chief rays are seen, forming the edges of the great east equatorial wing of the corona. A fainter ray runs nearly parallel to the great N.E. ray. The rays on the E. are traceable to about 1° from the Moon's limb.

No. 2. Neither so dense nor so clear as No. 1, nor can the rays be traced quite so far upon it.

No. 3. The sky glare has come up on this plate, which in consequence shows less than Nos. 1 and 2.

No. 4. Definition poor.

No. 5. Definition poor.

No. 6. Very dense image; the rays on the west can be traced to more than 2° from the Moon's limb.

The Dallmeyer R.R. lens was focussed by repeated photographs of star fields, especially of the field of the Southern Cross.

Development of the Photographs.—Owing to my having been taken ill on May 21 with a severe attack of malarial fever, the work of

developing the photographs was almost entirely carried out by Mr. Walter and by Mrs. Maunder, to whom the expedition is therefore greatly indebted for the results obtained. The weather being hot and damp, much difficulty was experienced with the different plates tested by way of experiment before the day of the eclipse. Finally the following developer was adopted and found to work well. In spite of its excessive dilution, it was found that fog resulted if its strength was materially increased.

Solution A.

Metol	40 grains.
Hydroquinone	50 „
Sulphite of soda	120 „
Bromide potassium	15 „
Water	20 ounces.

Solution B.

Caustic soda	100 grains.
Water	20 ounces.

For development half an ounce was taken of each stock solution and made up with water to 30 ounces, 15 drops of a 10 per cent. solution of bromide of potassium being added. During development this solution was strengthened by the gradual addition to it of a solution composed of 1 ounce of each of the two stock solutions made up with water to 10 ounces. But the stronger solution was seldom added to the weaker in a greater proportion than 1 to 15. The average time of development was 75 minutes.

Photographs of the Partial Phase.—These were taken with the photo-heliograph of the Royal Alfred Observatory, Mauritius, already described. The aperture for this purpose was reduced to $1\frac{1}{2}$ inches, and the exposure given by the drop-slit in the primary focus. Mr. T. F. Claxton made the exposures, and called the times by Kullberg 3400 to Bombardier Cox, who entered them. There being only three plate-carriers available for this work, Mr. C. Garrioch remained in the dark room to change the plates, and Bombardier Truman ran to and fro with them. The plates used were Cadett and Neale's ordinary lantern plates. The times of exposure were as follows :—

Before Totality.

No.	Observed times.			True local times.		
	h.	m.	s.	h.	m.	s.
1	7	8	19·8	7	7	19·4
2	7	10	19·8	7	9	19·4
3	7	14	5·3	7	13	4·9
4	7	18	1·3	7	17	0·9
5	7	22	46·3	7	21	45·9
6	7	28	47·3	7	27	46·9
7	7	31	7·8	7	30	7·4
8	7	35	6·2	7	34	5·8
9	7	38	47·3	7	37	46·9
10	7	42	43·0	7	41	42·6
11	7	44	32·4	7	43	32·0

After Totality.

12	8	2	48·8	8	1	48·4
13	8	3	43·3	8	2	42·9
14	8	11	44·5	8	10	44·1
15	8	12	41·0	8	11	40·6

Plate 16 had three exposures made upon it, the clock of the cœlostæt being stopped in order to give the means for determining the orientation of the plates.

No.	Observed times.			True local times.		
	h.	m.	s.	h.	m.	s.
17	8	19	40·0	8	18	39·6
18	8	22	34·3	8	21	33·9
19	8	24	25·7	8	23	25·3
20	8	26	44·3	8	25	43·9
21	8	31	28·6	8	30	28·2
22	8	32	33·2	8	31	32·8
23	8	34	40·5	8	33	40·1
24	8	36	24·5	8	35	24·1
25	8	39	56·3	8	38	55·9
26	8	42	7·5	8	41	7·1
27	8	45	13·2	8	44	12·8
28	8	48	15·4	8	47	15·0
29	8	50	33·7	8	49	33·3
30	8	53	8·2	8	52	7·8
31	8	54	5·0	8	53	4·6
32	8	59	50·2	8	58	49·8

The Director of the Royal Alfred Observatory has presented to the *Astronomer Royal* the photographs of the eclipse which he took with

the Observatory photoheliograph, both the photographs which he took of the corona during totality and the above photographs of the partial phase. The former I myself brought home to the Royal Observatory, Greenwich; the latter will be sent at the first available opportunity.

I took no observations myself of temperature during the eclipse, the Director of the Royal Alfred Observatory having made arrangements for full meteorological observations being taken both at the Observatory and at several other stations, not only in Mauritius, but also on other islands in the Indian Ocean.

“Preliminary Note on Observations of the Total Solar Eclipse of 1901 May 18, made at Pamplémousses, Mauritius.” By (Mrs.) A. S. D. MAUNDER. Received October 23,—Read at Joint Meeting of the Royal and Royal Astronomical Societies, October 31, 1901.

Having observed successfully the eclipses of 1898 January 22, in India, and 1900 May 28, in Algiers, and my husband having been sent to Mauritius as the representative of the Royal Observatory, Greenwich, to observe the eclipse of May 18 last, I determined to accompany him, and to take with me the instruments which we had used in 1900. By the great kindness of Mr. G. J. Newbegin, F.R.A.S., I was also furnished with a $4\frac{1}{4}$ -inch Cooke photo-visual telescope of 71 inches focus.

As my husband fixed his observing station at the Royal Alfred Observatory, Pamplémousses, and as the Director, Mr. Claxton, gave me every facility, I set up my instruments in the Observatory grounds. The Newbegin telescope was mounted upon the equatorial stand of the photoheliograph which was left vacant, the Mauritius photoheliograph having been dismantled in order that it might be used in connection with the 16-inch cœlostæt of the official Greenwich Expedition. The photoheliograph dome stands on the east of the Observatory grounds, and about 150 feet to the west of the dome I set up the Waters equatorial lent to my husband by the Royal Astronomical Society, which I used to carry two little cameras, each with a $1\frac{1}{2}$ -inch Dallmeyer Stigmatic lens. About 105 feet further west the camera belonging to the Waters telescope was firmly fixed, pointing directly to the Sun, and by its side the Niblett lens belonging to the British Astronomical Association, a photographic lens 4 inches in diameter, and of 34 inches focus. This was likewise fixed rigidly. Mr. Nevil Maskelyne, F.R.A.S., also lent me his kinematograph, and this was mounted a few feet further south. The management of these instruments during the eclipse was very kindly undertaken by several friends, and I was

further helped by three non-commissioned officers who had volunteered to assist in the eclipse observations. The entire party was arranged as follows :—

Personnel.

Mr. A. Walter : Newbegin photo-visual telescope, 71 inches focus.

Bombadier A. H. Thorne : Recorder to Mr. Walter.

Mrs. Walter Maunder : Dallmeyer Stigmatic lens twin camera, 9 inches focus.

Sergeant J. H. Tyson : Recorder to Mrs. Maunder.

Lance-Corporal A. Balgué : Waters camera, 8 feet equivalent focus.

Mr. G. H. Ireland : Kinematograph. The kinematograph gave no result, the film tearing across before totality was reached.

(Mr. Ireland brought a camera of about 2 feet focus, which was mounted as a stationary camera, and six exposures were made with it by Mr. T. W. Hanning.)

I add here a brief description of the photographs obtained :

1. *The Dallmeyer Stigmatic Twin Camera.*

These lenses are each $1\frac{1}{2}$ inches aperture, 9 inches focus, and give a field nearly 40° in diameter on a plate 16 cm. square. I procured one of these lenses in 1897, on account of its great field, in order to photograph the Milky Way, and it being the only photographic lens I then possessed, I took it to India in 1898. Exposures of 20 seconds made with it upon the corona of that year, recorded the coronal extensions much further from the Sun than they had been photographed before ; in one instance to a distance of 13.9 lunar radii from the Sun's centre. My purpose on this occasion was to repeat the Indian exposures of 20 seconds, and also to try the effect of exposures longer still. I therefore gave two exposures with each camera—one of 2 minutes and one of 20 seconds.

No. 1.—From 30 seconds to 150 seconds. Exposure 120 seconds.
Sandell Triple-coated plate.

A fairly dense negative. The chromosphere has come up as a very brilliant ring, but the motion of the Moon across the Sun's disc during the very considerable exposure is very apparent. Of course no detail of the inner corona is seen on such a small image, except the large prominence on the east limb of the Sun, but the outer corona is fairly defined, tapering in long rod-like rays which on the east side of the corona reach a distance from the Sun of about five lunar radii, and on the west side of about six.

No. 2.—From 30 seconds to 150 seconds. Exposure 120 seconds.
Imperial Special Rapid plate.

This negative is not so dense nor so well defined as No. 1. The

sky glare has come up, and the extensions are not so considerable as in the former case.

No. 3.—From 170 seconds to 190 seconds. Exposure 20 seconds.
Sandell Triple-coated plate.

The image of the corona is fairly dense, but the sky glare has not come up so much as in the previous cases, and it shows as great extension as No. 1.

No. 4.—From 170 seconds to 190 seconds. Exposure 20 seconds.
Imperial Special Rapid plate.

This is a weak image, and has no points to recommend it.

(Nos. 3 and 4 have a double image on them, due probably to the cameras not having settled down after the plate-carriers were changed.)

2. The Niblett Lens.

This lens is of 4 inches aperture and 34 inches focus, fitted with a revolving drum carrying 5-inch by 4-inch plates.

No. 1.—Exposed at second contact. Exposure about $\frac{1}{4}$ second.
Imperial Fine Grain plate.

This photograph was taken at second contact, and several "beads" of sunlight are seen. Nevertheless, not only is there a great deal of detail to be seen in the middle corona, but on the west side a coronal ray can be traced to two lunar radii, and on the east side to a somewhat greater distance. The image is a fairly dense one. The image is not large enough, nor was the exposure sufficiently short to show any detail in the prominences or chromosphere, but the polar rays and the "combing out" of the equatorial wings are well seen.

No. 2.—Exposed at 40 seconds. Exposure about $\frac{1}{4}$ second. Imperial Ordinary plate.

A fairly dense image, showing the polar rays and middle corona well.

No. 3.—Exposed at 80 seconds. Exposure about $\frac{1}{4}$ second. Imperial Special Rapid plate.

The image is not so dense as in the previous cases, but the coronal rays can be traced to a greater distance, as can also the polar rays.

No. 4.—Exposed at 120 seconds. Exposure about $\frac{1}{4}$ second.
Imperial Special Rapid plate.

A weak faint image, under-developed. No detail shown.

No. 5.—Exposed at 160 seconds. Exposure about $\frac{1}{4}$ second.
Imperial Ordinary plate.

Under developed, only the chromospheric ring seen.

No. 6.—Exposed at 200 seconds. Exposure about $\frac{1}{4}$ second.
Imperial Fine Grain plate.

A good image, not very dense, showing the middle corona and polar rays well.

3. *The Neubegin Telescope.*

This lens is a Cooke photo-visual objective of $4\frac{1}{4}$ inches aperture and 71 inches focus. The photographs were taken on quarter plates. They form an exceedingly fine series of fourteen, taken on different makes of plate and with differing exposures. They bear magnification well up to the limit we have yet tried, namely, to one giving 4 inches to the Sun's diameter. Every photograph in this series was successful.

No. 1.—Exposed 7 seconds after commencement of totality.
Exposure instantaneous. Plate, Lumière Orthochromatic,
Series A.

This is a not very dense negative, and shows the prominences on the east side of the Sun, and the very complicated structure of the corona surrounding them. From the large prominence on the Sun's east equatorial limb two very marked and irregular black rifts or streaks cross the coronal wing and seem to broaden as they get further away from the Sun. A similar single streak leaves the Sun's surface to the south of the large prominence in the south-east.

No. 2.—Exposed at 21 seconds after commencement of totality.
Exposure, 2 seconds. Plate, Imperial Fine Grain Ordinary.

A denser negative than No. 1, showing great extension of the polar rays. In the northern polar rays is seen a forked dark marking cutting the rays obliquely.

No. 3.—Exposed at 32 seconds after the commencement of totality.
Exposure 4 seconds. Plate, Imperial Fine Grain Ordinary.

The dark marking in the north polar rays is clearly seen. There is a greater extension on the north-western side of the corona than in the two previous photographs.

No. 4.—Exposed at 45 seconds after commencement of totality.
Exposure 6 seconds. Plate, Imperial Fine Grain Ordinary.

The corona is distinctly larger than on the previous photographs, and the dark marking cutting the north polar rays is very clearly seen.

No. 5.—Exposed 1 minute after commencement of totality. Exposure 8 seconds. Plate, Imperial Fine Grain Ordinary.

A negative very similar to No. 4.

No. 6.—Exposed at 77 seconds after commencement of totality. Exposure 10 seconds. Plate, Imperial Fine Grain Ordinary.

No. 7.—Exposed at 94 seconds after totality. Exposure 6 seconds. Plate, Imperial Ordinary.

This negative is perhaps not so well defined as the others.

No. 8.—Exposed at 107 seconds after totality. Exposure 6 seconds. Plate, Imperial Ordinary.

This negative is not very dense, but the corona is very large, and it shows a greater extension of the coronal rays than any of the previous ones.

No. 9.—Exposed at 126 seconds after totality. Exposure 10 seconds. Plate, Imperial Fine Grain Ordinary.

No. 10.—Exposed at 144 seconds after totality. Exposure 8 seconds. Plate, Imperial Fine Grain Ordinary.

No. 11.—Exposed at 162 seconds after totality. Exposure 6 seconds. Plate, Imperial Fine Grain Ordinary.

On this negative it is very clearly seen that the bulbous curves at the base of the great north-east wing are laid over and across other coronal curves in the background.

No. 12.—Exposed at 176 seconds after totality. Exposure 4 seconds. Plate, Imperial Fine Grain Ordinary.

This photograph shows perhaps best the corona in the south-west region. Here there seem to be a number of synclinal groups, which give the impression of not being all in the plane of the photograph, as usually appears to be the case in coronal negatives, but in different planes.

No. 13.—Exposed at 191 seconds after totality. Exposure 2 seconds. Plate, Imperial Fine Grain Ordinary.

The corona is here very restricted. In the south-west region the exposure does not seem to have been sufficient to bring up all the synclinal groups, and in consequence the south polar rays have the appearance of encroaching on this side.

No. 14.—Exposed at 202 seconds after the commencement of totality. Exposure instantaneous. Plate, Lumière Orthochromatic, Series A.

This negative is fainter than any of the others, but shows well the detail and prominence in the south-west region of the corona.

4. *The Waters Camera.*

This is a Dallmeyer portrait combination of 2 inches aperture and 8 feet equivalent focus, a Dallmeyer telephoto lens being used as a negative enlarger. The photographs were taken on half plates. The enlargement is rather too great for detail of any importance to be shown, and there is therefore no need to describe them in detail. Exposures of about a quarter of a second were given in all cases.

- No. 1.—Exposed at second contact. Plate, Imperial Special Rapid.
- No. 2.—Exposed at 40 seconds. Plate, Imperial Special Rapid.
- No. 3.—Exposed at 80 seconds. Plate, Imperial Flashlight.
- No. 4.—Exposed at 120 seconds. Plate, Imperial Flashlight.
- No. 5.—Exposed at 160 seconds. Plate, Imperial Flashlight.
- No. 6.—Exposed at 200 seconds. Plate, Imperial Special Rapid.

5. *The Pinhole Camera.*

This was a camera with pinhole aperture of $\frac{1}{32}$ of an inch, and the plate was placed at a distance of 30 inches. The plate was exposed immediately before second contact, and closed immediately after third. A small arc of sunlight is thus seen both on the east and west limbs of the Moon. A coronal ring, about 4 minutes in greatest height, is seen connecting these arcs. The little photograph has of course no scientific value, but as being the only instance of a photograph of the corona taken with a pinhole, it may rank as a curiosity. The ratio of aperture to focal length was as 1 to 960, and the exposure given to the plate would correspond to one of $\frac{1}{125}$ th of a second, as given with the Newbegin. The plate used was an Imperial Special Rapid.

6. *Photographs of the Landscape.*

At my request M. Alexander Rambert, a professional photographer of Mauritius, took a series of photographs of the landscape during the coming on and passing off of the shadow. The same exposure was given in every instance, the plates were all from the same batch, and they were developed at one and the same time, in the same bath and for the same length of time. The exposures were made at regular intervals of 5 minutes, the first being taken $27\frac{1}{2}$ minutes before mid-totality, the last $27\frac{1}{2}$ minutes after. A comparison of the plates taken before totality with those taken at corresponding times after, shows a slight but appreciable increase in density in the later plates as compared with the earlier, but perhaps not more than would be accounted for by the increase in altitude of the Sun.

November 30, 1901.

Anniversary Meeting.

Sir WILLIAM HUGGINS, K.C.B., D.C.L., President, in the Chair.

(The full Report of the Anniversary Meeting, with the President's Address and Report of Council, will be found in the 'Year-book' for 1902.

The Account of the Appropriation of the Government Grant and of the Trust Funds will also be found in the 'Year-book'.)

December 5, 1901.

Sir WILLIAM HUGGINS, K.C.B., D.C.L., President, in the Chair.

Mr. Hector Munro Macdonald was admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The President announced that he had appointed as Vice-Presidents for the ensuing year—

The Treasurer.

Dr. Armstrong.

Dr. Blanford.

Professor Emerson Reynolds.

The following Papers were read :—

- I. "On the Spontaneous Ionisation of Gases." By C. T. R. WILSON, F.R.S.
- II. "Notes on Quantitative Spectra of Beryllium." By Professor W. N. HARTLEY, F.R.S.
- III. "Notes on the Development of *Paludina vivipara*, with Special Reference to the Urino-genital Organs and Theories of Gastropod Torsion. Preliminary Note." By Miss I. M. DRUMMOND. Communicated by Professor WELDON, F.R.S.
- IV. "Preliminary Account of the Prothallium of *Phylloglossum*." By Professor A. P. W. THOMAS. Communicated by Prof. HOWES, F.R.S.

"The Development of *Echinus esculentus*." By E. W. MACBRIDE, M.A., D.Sc., Professor of Zoology in McGill University, Montreal. Communicated by ADAM SEDGWICK, F.R.S. Received November 20,—Read November 28, 1901.

The present communication is to be regarded as a brief preliminary account of an investigation which has occupied my attention for the last three years. The object of this investigation was to examine by means of modern methods the manner in which the various organs of the adult *Echinus* are fashioned out of the corresponding structures of the *Pluteus* larva, and this object has been at last successfully accomplished, and the most important results are briefly described in the present paper.

It was necessary in the first instance to rear the larvæ through the later stages of development in considerable quantities, and the means by which this was accomplished have already been described in two short communications.*

The complete development from fecundation until the conclusion of metamorphosis occupies about 45 days, a period which closely agrees with that required for the development of *Echinocyamus pusillus* as determined by Théel.†

The fertilised eggs become at the end of 24 hours free-swimming ciliated blastulæ; at the end of 48 hours, by the invagination of one side, they are transformed into gastrulæ. The larvæ now assume a somewhat prismatic form, one side becoming flattened and even concave, whilst the opposite one becomes developed into a more or less rounded hump. On the flattened side, about 3 days after fertilisation, the stomodæum makes its appearance as a wide, shallow invagination. The gut has already been differentiated into the three portions, viz., œsophagus, stomach, and intestine, and from the blind apex of the first of these, which has not as yet met the stomodæum, the cœlomic rudiment is given off in the form of a flattened sac. This sac rapidly becomes bilobed and then divided into two, the right and left cœlomic vesicles.

The longitudinal ciliated band, which is the organ of locomotion, makes its appearance at this time as a thickening round the edge of the concave side of the larva, and at the same time the adoral band appears. This latter, which has the form of a V-shaped loop, projecting

* "Studies in the Development of Echinoidea.—I. The Larvæ of *Echinus esculentus* and *Echinus miliaris*," 'Quart. Journ. Micro. Sci.,' vol. 42, 1899; "The Rearing of Echinoderm Larvæ," 'Journ. Mar. Biol. Assoc.,' vol. 6, No. 1, 1900.

† "The Development of *Echinocyamus pusillus*," 'Trans. Roy. Soc. of Upsala,' 1892.

into the œsophagus, can first be discerned at the age of 4 days after the alimentary canal is completed by the opening of the stomodæum into the œsophagus. It is formed both from the lining of the stomodæum, which is of course ectoderm, and from the endoderm of the œsophagus. This is important, in view of the circumstance that it has been compared to the V-shaped endostyle of the larva of *Amphioxus* (see fig. 1).

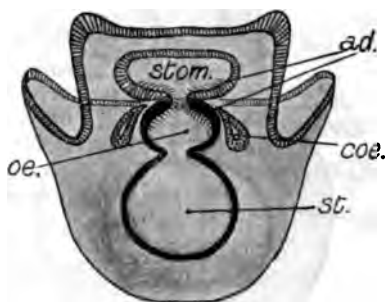


FIG. 1.—Diagrammatic View of a Larva about 4 days old, seen from in front.
ad., adoral band of cilia; *coe.*, celomic vesicle; *œ.*, œsophagus; *st.*, stomach;
stom., stomodæum.

By this time, through the formation of outgrowths from the longitudinal ciliated band, the first four arms of the *Pluteus* have been formed. Even at this early period the left celomic vesicle is wider and more thin-walled than the right, and at $4\frac{1}{2}$ days a vertical tubular outgrowth is formed from it, which reaches and fuses with the ectoderm, and in this way the primary pore-canal and madreporic pore are established. These are distinctly structures belonging to the left side. No trace of corresponding structures on the right side was ever observed.

The celomic vesicles grow backwards along the sides of the stomach; that on the left side shows an anterior dilatation into which the pore-canal opens, followed by a constriction and a posterior dilatation. When the larva has reached the age of 8 or 9 days, this posterior dilatation divides into two (fig. 2), the front part being the rudiment of the hydrocele, whilst the hind part will form the left posterior celom, out of which a large portion of the adult body-cavity is formed. The anterior dilatation is the so-called "ampulla" of the stone-canal, and the neck connecting it with the hydrocœle is the stone-canal itself (fig. 3).

About this time the rudiments of the 5th and 6th *Pluteus* arms make their appearance, and shortly afterwards the right celomic vesicle is divided into an anterior and a posterior part. This occurs when the

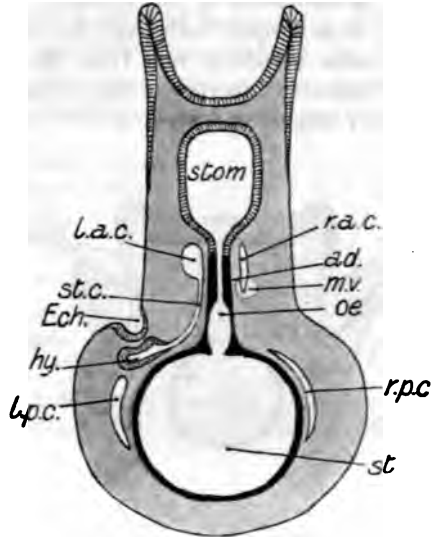


FIG. 2.—Diagrammatic Longitudinal Frontal Section of a Larva about 18 days old.

ad., *æ.*, *st.*, *stom.*, as before; *Ech.*, invaginated ectoderm about to form the oral disc of the future Echinus; *hy.*, hydrocele; *l.a.c.*, left anterior coelom (ampulla of stone-canal); *l.p.c.*, left posterior coelom; *mv.*, madreporic vesicle (right hydrocele—this is not in the plane of the figure); *r.a.c.*, right anterior coelom; *r.p.c.*, right posterior coelom.

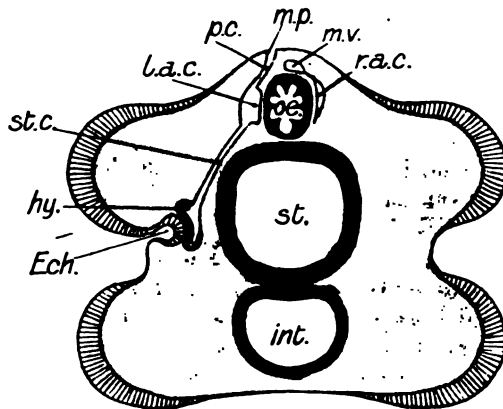


FIG. 3.—Diagrammatic Transverse Section of a Larva about 18 days old.

Names as before; in addition, *int.*, intestine; *mp.*, primary madreporic pore; *p.c.*, pore canal.

larva is 11 days old, so that by that time the coelom of the larva has completed its transverse segmentation (fig. 4, *d*).

About 13 days a hollow pouch-like invagination of the ectoderm takes place in the region where the hydrocoele is situated, and this is the first trace of the ectoderm of the oral disc of the *Echinus* (fig. 2). At the same time four horizontally placed festoons of the longitudinal ciliated band become separated from the rest and form the "ciliated epaulettes," which are the main organs of locomotion of the larva in the later stages of its development (see fig. 3).

At 16—17 days an outgrowth takes place from the hinder end of the right anterior coelom. It is directed dorsally, and its free end thickens into a nodule of cells which for a brief period is connected with the rest with a string of cells which soon breaks. The nodule then becomes hollowed out, and forms a perfectly closed vesicle lying beside the "ampulla" of the stone-canal. It is the rudiment of the "madreporic vesicle" or "espace sous-madreporique" of the adult, and represents, as I have shown in the case of *Asterina gibbosa*, a rudimentary fellow of the hydrocoele. We may interpret the transient cord of cells as a right stone-canal (see figs. 2 and 3).

Reviewing the development thus far described, it appears that the coelom of the right side of the larva undergoes similar changes to those experienced by the coelom of the left side, but they are slower in their development and this agrees with the facts observed in the case of *Asterina gibbosa*.

The important fact that the coelom of the *Pluteus* underwent transverse segmentation was first made known by Bury* in 1889. His observations were, however, confined to specimens picked out of collections of Plankton, and the series of developmental stages which he obtained was not without considerable gaps. On this account the

* "Studies in the Embryology of Echinoderms," 'Quart. Jour. Micro. Sci.,' vol. 29, 1889.



FIG. 4.— Five Diagrams showing the Changes undergone by the Coelomic Vesicles of the Pluteus Larva. Names as before.

correctness of his views has been often questioned even by Théel.* It affords me much pleasure to be able to completely confirm Bury's views; every stage of the process of the transverse division of the coelom has been observed by me.

At 19—20 days the ectodermic invagination on the left side of the larva has become flask-shaped, and its floor is now in close contact with the hydrocoele. The latter has now taken on the form of a flattened pentagon (fig. 6). It never has the form of an open hoop, so characteristic of the corresponding organ in Asteroidea, Ophiuroidea, and Holothuroidea.

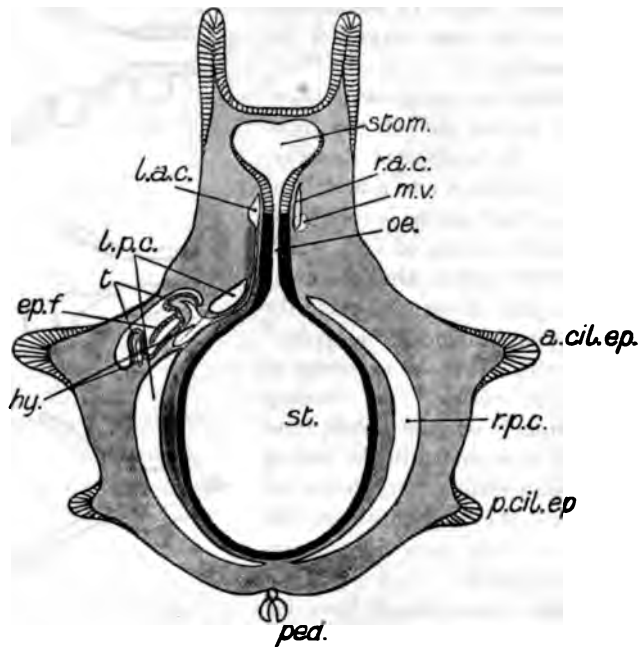


FIG. 5.—Diagrammatic Longitudinal Section of a Larva about 23 days old.

Names as before. In addition, *a.cil.ep.*, anterior ciliated epaulette; *p.cil.ep.*, posterior ciliated epaulette; *ep.f.*, epineural fold; *ped.*, apical pedicellaria; *t.*, primary tube-feet of the Echinus.

About 21 days the rudiments of the last Pluteus arms (the 7th and 8th) appear, and the pentagonal hydrocoele sends out at its corners five projections which are covered by corresponding elevations of the ectoderm. These are the rudiments of the radial canals of the adult, and their terminations form the five primary azygous tube-feet of the young Echinus which correspond to the ocular tentacles of an Asterid.

* *Loc. cit.*

At the same time the ectodermic invagination becomes completely closed: its floor consists of a thick layer of cells which is moulded over the outgrowths of the developing hydrocoele, but its walls and roof remain very thin. Its cavity will be called the amniotic cavity.

The further development of the Pluteus consists chiefly in the enlargement of the hydrocoele and of its outgrowths, and of course of the ectoderm covering the latter, and of the amniotic cavity into which they project.

At about 23 days five interradial ridges of the ectoderm, which covers the hydrocoele, make their appearance, alternating with the five primary tentacles. The sides of these ridges grow out into over-arching lamellæ, which soon meet one another, and in this way five epineural canals are formed, opening into a central epineural space (see figs. 5 and 6).

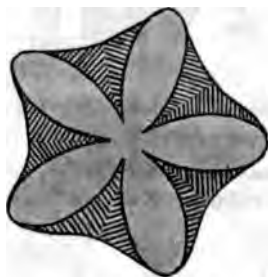


FIG. 6.—Plan of the Hydrocoele (Budiment of the Water-vascular System) of a Larva about 24 days old.

The wedges (distinguished by cross-hatching) are the epineural folds, which eventually meet one another laterally and form a complete covering over the hydrocoele.

At 26 or 27 days the left posterior coelom has extended to the posterior apex of the hump, and has there met the right posterior coelom, forming a thin vertical mesentery. Just above this mesentery the rudiment of the first pedicellaria appears as a little elevation filled with amœbocytes (fig. 5). A little later the rudiments of two other pedicellariæ appear on the right side—one situated dorsally, the other ventrally.

The left posterior coelom, which was at first situated entirely posterior to the hydrocoele, has by this time grown completely round it in a circle, and in the axis of this circle the stone-canal still connects the "ampulla" or anterior coelom and the hydrocoele (fig. 5).

At 27—28 days the rudiments of the masticatory system of the so-called "Aristotle's lantern" make their appearance. These are five outgrowths of the left posterior coelom, having the shape of inverted

wedges, which alternate with the five radial canals which have been developed from the hydrocele (fig. 7). These outgrowths soon lose

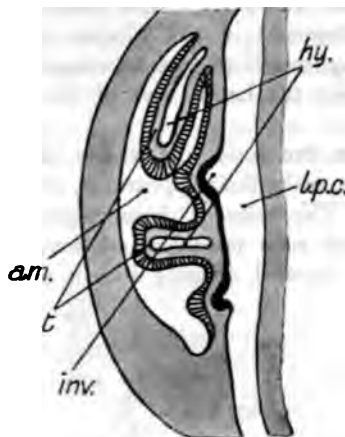


FIG. 7.—Diagrammatic Section of the developing Oral Disc of the future *Echinus* in a Larva 26 days old.

Names as before. In addition, *am.*, amniotic cavity; *inv.*, invaginations of coelomic epithelium, which are the rudiments of the perihæmal spaces and of the "lantern coelom."

their connection with the coelom, and become converted into closed sacs. From each of them a narrow outgrowth sprouts out, which insinuates itself between the water-tube and the ectoderm of the base of the tentacle. In this way the radial perihæmal canals are formed. The main body of the sac, however, forms a tooth-sac (fig. 8). From its floor an upstanding projection is formed, which is the root of the tooth, whilst from its sides the alveoli, which constitute the jaw, are formed. Calcification, however, proceeds in these parts only to a very slight extent till after the metamorphosis.

By the time the larva has attained an age of 32—33 days, the primary tube-feet have become long, and have acquired their suckers, and, in addition, a curious little sensory prominence, consisting of elongated ectoderm cells situated in the centre of the sucker; the three pedicellariæ mentioned above are fully developed, and the four primary ciliated epaulettes have grown so as to almost meet one another, and thus form a complete circle. In addition, two secondary ciliated epaulettes have been formed in a more posterior position (see fig. 5); these extra epaulettes are characteristic of *Echinus esculentus* as opposed to *Echinus miliaris*.

The larva continues to grow up to 36—38 days after fecundation. By that time several important changes have taken place. The hydro-

coele and amniotic cavity have grown so as to occupy the entire side of the larva; the tube-feet are long, and show wriggling movements. Spines have commenced to make their appearance; one or two small quadrangular rudiments are situated at the sides of the posterior pedicellaria, but, in addition, there are ten conical outgrowths of the oral disc of the Echinus, *i.e.*, of the floor of the amniotic cavity, alternating with the radial canals.

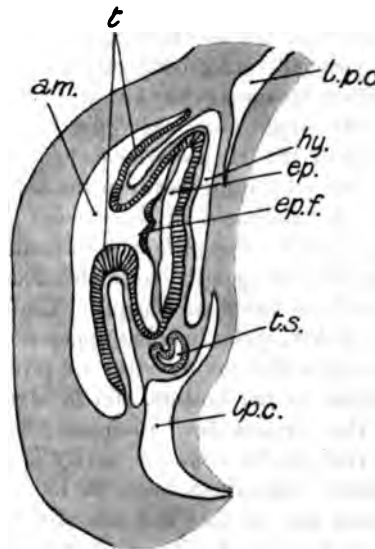


FIG. 8.—Diagrammatic Section of the developing Oral Disc of the future Echinus in a Larva 30 days old.

Names as before. In addition, *ep.*, epineural space; *ep.f.*, epineural fold; *t.s.*, tooth-sac (developed from the invagination of cœlomic epithelium shown in fig. 7).

The first trace of the adult mouth and œsophagus appears as an invagination of the oral disc, indenting the centre of the hydrocoele. At a later period it completely perforates the hydrocoele, thus converting it from a flattened plate into a ring. The adult œsophagus is thus shown to be a stomodæum. The stomach loses its globular form, and becomes flattened on the side towards the hydrocoele.

About this time the first trace of the so-called "blood-system" becomes apparent. From the beginning the wall of the gut has at no time been in actual contact with the cœlomic wall. Always there has intervened a layer of jelly with a few amœbocytes in it, the same substance which exists in much greater quantity between the outer wall of the cœlom and the ectoderm.

At about 36 days the jelly intervening between the cœlom and the

gut becomes profoundly altered in chemical composition. It acquires staining properties, but the amœbocytes contained in it remain glassy and clear. At the same time, the gut-cells become more difficult to stain, pointing clearly to the exudation of proteid matter from the gut-cells into the surrounding jelly. From this envelope of altered jelly the mesenteric and anti-mesenteric vessels of the adult are developed.

The metamorphosis commences at about 40—42 days. By that time the amniotic cavity, the hydrocoele, and the underlying left posterior coelom have grown so greatly that the posterior epaulettes and the pedicellaria in the centre of the aboral surface are pushed to one side. In other words, the preponderant growth of the organs of the left side, which in *Asterina gibbosa* becomes evident at an early period of development, here makes its appearance only just before metamorphosis. From the anterior end of the left posterior coelom a cellular rod grows out, burrowing into the jelly lying between the ampulla and the right hydrocoele. This is the rudiment of the "ovoid gland," or so-called "heart," which here, as in *Asterina gibbosa*, is in reality a genital stolon. The anus closes up, and the mouth closes also about the place where the stomodæum joined the larval œsophagus. The Pluteus arms have become shorter and thicker, and a hole has appeared in the roof of the amniotic cavity through which the tube-feet are protruded.

The larva now sinks to the bottom, and in a very short time the ciliated band and the "epaulettes" disappear, being devoured by amœbocytes. The roof of the amniotic cavity is entirely abolished, and the young *Echinus* creeps about freely on its five tube-feet. The rudiments of a second pair of tube-feet are already present in each radius, but are not yet functional. The arched dorsal surface, with its spines and the three pedicellaria, is at this time greater in extent than the ventral ambulatory surface, so that it may be said that the young *Echinus* passes through an Asterid stage in development.

The whole ambulacral surface, however, grows rapidly in comparison with the dorsal surface; the terminal tube-feet become smaller, and are eventually sunken in grooves in the test. They become adherent to the sides of these grooves, and so the long flexible tube-foot of the just metamorphosed *Echinus* becomes changed into the insignificant ocular nodule of the adult.

Compared with the development of *Asterina gibbosa*, the development of *Echinus esculentus* must be regarded as being very much modified; nevertheless, all the main features of the development of *Asterina* can be recognised in *Echinus*.

“On the Spontaneous Ionisation of Gases.” By C. T. R. WILSON, M.A., F.R.S., Fellow of Sidney Sussex College, Cambridge. Received October 22,—Read December 5, 1901.

It was shown by Geitel,* and independently by the writer of this paper,† that there is a continuous production of ions in air contained in a closed vessel, even when it is not exposed to any known ionising agent. The present communication contains the results of measurements of the relative rates of production of ions under such conditions in air and certain other gases.

The relative ionisations (the term “ionisation” being used in the sense of “rate of production of ions”) were obtained by a comparison of the “saturation” currents in the various gases. The apparatus used for this purpose differed only in minor details from that described and figured in a previous paper.‡

The vessel in which the ionisation was measured was a glass bulb 5.5 cm. in diameter, thinly silvered internally. The conducting system, from which took place the loss of charge to be measured, consisted of a brass wire with a narrow strip of gold leaf attached, the wire being fixed by means of a small bead of sulphur to a brass rod passing through a plug of sealing-wax in the neck of the bulb. This supporting rod was kept at a constant potential by means of a condenser, consisting of zinc plates embedded in sulphur. Electrical connection could be made when required between the leaking system and the supporting rod by means of a strip of watch balance-spring, soldered at its upper end to the supporting rod, and with its lower end bent so as to form a bridge over the sulphur bead. By approaching a magnet to the neck of the bulb the free end of the spring could be brought into contact with the upper end of the brass wire carrying the gold leaf.

The position of the gold leaf was read by means of a microscope provided with an eye-piece micrometer-scale. On the portion of the scale used, one scale division corresponded to 0.31 volt, as tested by means of a Clark cell. An initial potential of about 90 volts was used in the experiments.

To make a determination of the rate of leak, the brass wire and gold leaf were brought to the potential of the supporting rod by making momentary electrical connection by means of the magnetic contact maker. The time indicated by a watch at the moment of transit of the gold leaf across the next scale division was then observed; the moment of transit across a lower scale division about one hour later was also observed. Contact was then made a second time and the position of the gold leaf again read. The time taken by the gold leaf to traverse a certain whole number of scale divisions was thus deter-

* ‘*Physik. Zeitschrift*,’ 2 Jahrgang, No. 8, pp. 116–119.

† ‘*Camb. Phil. Soc. Proc.*,’ vol. 11, p. 32; ‘*Roy. Soc. Proc.*,’ vol. 68, pp. 151–161.

‡ ‘*Roy. Soc. Proc.*,’ vol. 68, p. 155.

mined, and the constancy of the potential of the supporting rod at the same time tested.

The number of scale divisions traversed during the course of a determination of the leak did not in any case exceed ten; in the case of the smallest rates of leak measured only one scale division was traversed. As one scale division corresponded to 0.31 volt, the potential of the leaking system never fell more than about 3 volts below that of the supporting rod.

The potential of the supporting rod did not itself remain absolutely constant, but fell generally from 1 to 2 volts in the hour, unless a small additional charge was given to the condenser during the course of the experiment; this was done in the case of the smaller leaks. The correction to be applied for a fall of potential of the support corresponding to a given number of scale divisions was determined by first making contact by means of the magnet, then reducing the potential of the supporting rod sufficiently to move the gold leaf across a whole number of scale divisions, and again making contact and observing the new potential of the supporting rod. It was found that to make a change of potential in the leaking system corresponding to one scale division (the charge remaining constant) a change of potential of the supporting rod corresponding to 5.4 divisions was required; or the correction to be applied for a fall of potential of the supporting rod corresponding to one scale division amounted to 0.18 of a division.

The error due to want of perfect insulating power in the sulphur bead was certainly very small. Even when the supporting rod was earthed, so that the difference of potential between the two conducting systems separated by the sulphur was about 90 volts, the movement of the gold leaf, when the apparatus was exhausted, did not exceed one division per hour. Under the conditions of the actual experiment the difference of potential between the two systems never exceeded 3 volts, and with the apparatus exhausted no leak was observed.

The apparatus for the preparation and drying of the gases was sealed on to the bulb in which the leakage was measured, all joints being made with the blow-pipe. A mercury pump served to exhaust the apparatus before the introduction of the gases. The gases were dried by passing them slowly over phosphoric anhydride.

The air was admitted through a cotton-wool filter. The carbonic acid was prepared by heating sodium bicarbonate. The hydrogen used in the first experiments was prepared from zinc and sulphuric acid; in the later experiments the gas thus obtained was absorbed by palladium, from which it was set free by heating. The sulphur dioxide was obtained from a "siphon" of the liquid substance. The chloroform vapour was obtained from the commercial substance. The temperature ranged from 20° to 24° C. The results are given in the tables which follow.

Air.

	Pressure in centimetres of mercury.	Leak, scale divi- sions per hour (corrected).	Leak/pressure.
(1)	76	8.0	0.105
(2)	68	7.3	0.107
(3)	57	6.3	0.111
(4)	51	5.3	0.104
(5)	40	4.9	0.122
(6)	27	3.45	0.128
(7)	16	2.0	0.125
(8)	10	1.2	0.12

Leak/pressure, mean of (5) (6) (7) = 0.125.

Hydrogen.

	Pressure in centimetres of mercury.	Leak, scale divi- sions per hour (corrected).	Leak/pressure.
(1)	77	1.75	0.023 { From Zn and H ₂ SO ₄ . " " palladium. " " " " " "
(2)	77	1.70	
(3)	77	1.67	
(4)	77	1.94	
(5)	70	1.6	0.023 " " "
(6)	68	1.5	0.022 From Zn and H ₂ SO ₄ .
(7)	43	0.96	0.022 " palladium.
(8)	36	0.7	0.020 " "

Leak/pressure, mean of (1)—(7) = 0.0225.

Relative leak (air = 1)..... = 0.184.

Carbonic Acid.

	Pressure in centimetres of mercury.	Leak, scale divi- sions per hour (corrected).	Leak/pressure.
(1)	49	8.7	0.178
(2)	41	7.7	0.188
(3)	35	7.1	0.203
(4)	28	6.1	0.218
(5)	23	4.8	0.209
(6)	17	3.3	0.194
(7)	9.7	1.9	0.196
(8)	4.6	1.1	0.24

Leak/pressure, mean of (3)—(6) = 0.211.

Relative leak (air = 1)..... = 1.69.

Sulphur Dioxide.

	Pressure in centimetres of mercury.	Leak, scale divi- sions per hour (corrected).	Leak/pressure.	
(1)	21.3	5.3	} = 5.5	0.258
(2)	21.3	5.7		
(3)	12.3	3.9		
(4)	12.3	3.9	} = 4.0	0.325
(5)	12.3	4.25		
(6)	7.3	2.3	} = 2.4	0.33
(7)	7.3	2.5		
(8)	7.3	2.4		

Leak/pressure, mean of (3)—(8) = 0.33.

Relative leak (air = 1) = 2.64.

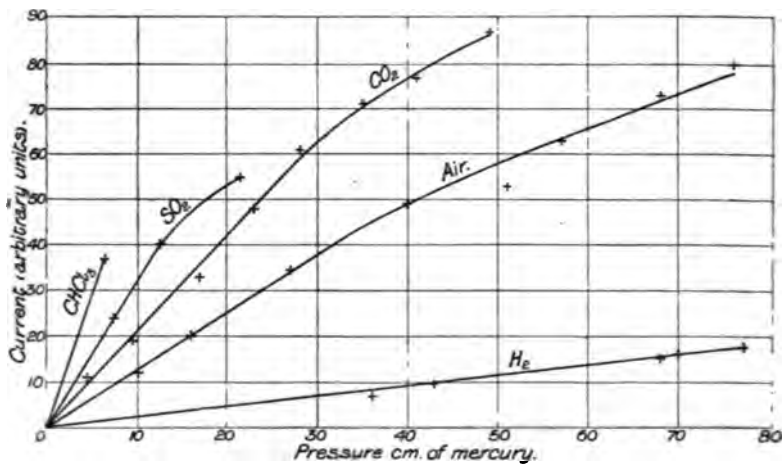
Chloroform.

	Pressure in centimetres of mercury.	Leak, scale divi- sions per hour (corrected).	Leak/pressure.
(1)	6.3	3.6 } = 3.7 3.8 }	0.59
(2)	6.3		

Leak/pressure, mean = 0.59.

Relative leak (air = 1) = 4.7.

These results are represented graphically below.



It will be noticed that except at the higher pressures the leak is proportional to the pressure. The falling off from this law at the higher pressures might be taken as indicating that the ionisation is due to radiation from the walls of only moderate penetrating power; it is possible, however, that at the higher pressures a sensible proportion of the ions recombine, so that the current is not a measure of the ionisation. There can be no doubt that, for the range of pressures for which the leakage is proportional to the pressure, we may consider that the saturation current was attained, and the rate of leak may be taken as a measure of the ionisation. It is for such pressures that the relative ionisations have been calculated. They are collected together in the table which follows.

Gas.	Relative ionisation.	Relative ionisation. Specific gravity.
Air	1·00	1·00
H ₂	0·184	2·7
CO ₂	1·69	1·10
SO ₂	2·64	1·21
ChCl ₃	4·7	1·09

If we except hydrogen, the ionisation is seen to be very nearly proportional to the density of the gas. The deviations in the case of the other gases in fact hardly exceed what might be attributed to experimental errors, for variations amounting to as much as 10 per cent. occurred in the determination of the rate of leak for a given gas. The departure from the law in the case of hydrogen cannot be attributed to experimental error, nor is it due to want of purity of the gas, as is shown by the agreement in the results obtained with the gas prepared by different methods.

The results obtained for the relative ionisation occurring spontaneously in all the gases tried, including hydrogen, show a remarkable similarity to those obtained by Strutt* in the case of the same gases when under the action of the Becquerel rays, as the following table shows (p. 282).

It was pointed out by Strutt that the conductivity under the action of the rays is nearly proportional to the density of the gas, hydrogen, however, being an exception. As will be seen, the results I have obtained for the spontaneous ionisation agree more nearly with those obtained by Strutt for gases exposed to radium radiation than for the same gases under the action of polonium rays.

An account of measurements of the loss of electricity from a charged conductor suspended in a closed vessel containing various

* 'Phil. Trans.,' A, vol. 196, pp. 507-527.

gases was published by Matteucci* in 1850. He found the loss to be the same in air, carbonic acid, and hydrogen. Warburg,† in 1872, found the loss in air and carbonic acid to be equal, and in hydrogen about half as great. They used much larger vessels, and it is probable that in Warburg's experiments the saturation current was not approached, so that the leakage was not even approximately proportional to the ionisation; it is perhaps in this way that the discre-

Relative Conductivity (Air = 1).

Gas.	Radium rays (penetrating type).	Polonium rays.	Spontaneous ionisation.
Air	1·00	1 00	1·00
Hydrogen	0·157	0·226	0·184
Carbonic acid.....	1·57	1·54	1·69
Sulphur dioxide	2·32	2·04	2·64
Chloroform	4·89	4·44	4·7

pancy is to be explained. In Matteucci's experiments, however, the leakage was independent of the potential; he was therefore using potentials high enough to produce the saturation current, and some other explanation is in this case required. One possible way out of the difficulty would be to suppose that the ionisation is to be explained by a slight radio-activity of the walls, the radiation being of only moderate penetrating power. In a large vessel all the radiation might be absorbed even by hydrogen, and in that case we should expect to find the same total number of ions produced in a given time whatever gas the vessel contained.‡ At the same time the similarity between the results obtained for the relative ionisations of different gases in the small vessel with those of the same gases exposed to the action of radium would be explained (the ionisation being due to a similar cause), as also would the departure at the higher pressures from the simple law connecting pressure and rate of leak, which holds for the lower pressures, for at the higher pressures the intensity of the rays might be considerably reduced by absorption before reaching the central portions of the vessel.

Until, however, further experiments have been made it would, I think, be premature to conclude that the ionisation is due to radiation from the walls of the vessel.

* 'Annales de Chimie et de Physique,' vol. 28, pp. 385-429.

† 'Annalen d. Physik u. Chemie,' vol. 145, pp. 578-599.

‡ As in gases exposed to uranium rays in a large vessel (Rutherford, 'Phil. Mag.,' vol. 47, p. 136, 1899).

"Notes on Quantitative Spectra of Beryllium." By W. N. HARTLEY, D.Sc., F.R.S., Royal College of Science, Dublin.
Received October 31,—Read December 5, 1901.

In a quantitative examination made in 1885 of all the known methods of separating beryllium from aluminium and from iron, the various precipitates obtained were dissolved and diluted to a known volume corresponding with the amount of bases in solution.

The solutions were spectrographically examined, and the photographs compared with others taken from solutions containing accurately weighed quantities of pure beryllia. The coil used was capable of giving a 5-inch spark in air. In place of a Leyden jar a pane of glass coated on each side with a square foot of tinfoil was used. The electrodes* were Ceylon graphite as in other experiments, the sole impurity in which was a trace of magnesium.

The following tabulated statement gives the wave-lengths of the lines, together with a description of the spectra photographed from solutions of different strengths (see next page).

The actual length of the line 2478·1, as rendered by solutions of 0·00001 per cent. and 0·000001 per cent. strength, is, in the former, 0·07, and, in the latter, 0·05 of an inch. The normal length of the line at this part of the spectrum is 0·22 of an inch. The quantity of substance yielding this spectrum is equivalent to one-millionth of a milligramme of beryllium. As I have pointed out in the case of magnesium,† so also is it with beryllium, that the sensitiveness of the spectrum reaction may be increased ten thousand-fold by using a larger coil and more powerful condenser, but leaving the striking distance between the electrodes unaltered. The coefficient of complete extinction was therefore practically not attainable for all the lines, or, in other words, the sensitiveness of the reaction is almost without limit.

It will also be seen from my description of the spectra, which have been quite recently re-examined, that the coefficient of extinction of the two lines λ 3130·3 and 2478·1 had not been reached by the dilution specified.

A number of thin sections of the Dublin granite containing microscopic crystals of hexagonal form were examined some years ago. The crystals were supposed to be apatite, but a very carefully executed analysis disclosed the fact that the proportion of phosphoric acid contained in 20 grammes of the rock was almost infinitesimal, and that on warming thin sections, in which the hexagonal crystals were visible, with nitric acid and ammonium molybdate, no deposit of yellow

* 'Phil. Trans.,' 1884, Part I, p. 49.

† 'Phil. Trans.,' 1884, Part II, p. 325.

Strength of Solutions or Degrees of Dilution shown by per cent. of Beryllium.

Beryllium wave-lengths (Rowland's scale).	1 per cent.	0.1 per cent.	0.01 per cent.	0.001 per cent.	0.0001 per cent.	0.00001 per cent.
3322.3	A continuous line	Half of the line is much weakened	A very short line	A faint indication	Extinct	Extinct.
3130.3	A continuous line	A continuous line	One-half of the line weakened	One-half of the line weakened	One-half the line still strong	Nearly one-half, still strong.
2649.8	A continuous line	Half of the line is weakened	A very short line	Very short line	A dot	A dot.
2493.6	A continuous line	Half of the line is weakened	Very short line	Very short line	A very faint dot	A dot, scarcely visible.
2478.1	A continuous line	A continuous line	A short line	A very fine short line	A very fine short line	A very fine short line.

ammonium phospho-molybdate could be detected by the microscope. A spectrographic analysis showed that these crystals were really beryls, and similar crystals a millimetre in length were picked out of the granite. They were found to contain between 10 and 11 per cent. of beryllia. Since then beryllia has been separated from the alumina of felspar obtained from the granite in Glen Cullen in proximity to a vein of coarse granite in which beryls were found by Dr. John Joly.

From numerous experiments on the analytical processes employed in the separation of beryllia from alumina, it was found that it remained combined with the sesquioxide bases in so persistent a manner as to lead to the belief that ordinary alumina might be found more often than not to contain traces of beryllia, particularly as there is no easily applied chemical test for detecting its presence in small quantities, nor a simple means of separating it. It has, however, been found that such is not the case, though gallium has been ascertained to be present in almost all minerals which contain aluminium. As they belong to the same group, the two elements aluminium and gallium may be expected to form isomorphous mixtures, which would account for their being so constantly associated in nature; but the position of beryllium in the periodic system of classification shows that a similar behaviour with that element is scarcely probable.

"Preliminary Account of the Prothallium of *Phylloglossum*."

By A. P. W. THOMAS, M.A., F.L.S., University College, Auckland, N.Z. Communicated by Professor G. B. HOWES, F.R.S. Received October 31,—Read December 5, 1901.

Our knowledge of the development of the Isosporous Lycopodinæ is still so incomplete that no apology seems necessary for the present preliminary statement. A special interest attaches to *Phylloglossum* since it has been recognised as a permanently embryonic form of Lycopod.* *Phylloglossum* is a genus with a single species—*P. Drummondii*—confined to Australia and New Zealand. The sporophyte generation is a small plant, growing from a tuber, which forms a tuft of a few cylindrical tapering leaves. In fertile plants the apex of the stem forms a peduncle, terminated by a cone or strobilus of small, scale-like, fertile leaves.

Träub has shown that the embryo of *L. cernuum* shows a remarkable likeness to a barren plant of *Phylloglossum*, for the first formed leaves have the character of the leaves of *Phylloglossum*; hence the term protophylls has been given to these structures, so different from the

* Bower, 'Phil. Trans.,' 1886, p. 676.

ordinary leaves of Lycopods. Further, the embryo of *L. cernuum* forms at a very early stage a tuber (protocorm), above which its prophylls rise. To this protocorm the tuber of *Phylloglossum* is apparently comparable, but in *Phylloglossum* it is not a passing embryonic structure, but is repeated annually on the formation of a new protocorm. Treub regarded the protocorm as the representative of a primitive structure originally possessed by the Pteridophytes, a structure which may have served an important part in the phylogeny of the higher plants, in enabling the sporophyte to attain an existence independent of the gametophyte.

The prothallia of *Phylloglossum* have been obtained growing naturally amongst the parent plants, but it is a significant fact that in most places, even where older plants are abundant enough, no prothallia could be discovered, though many whole days were spent in fruitless search. In three localities only were prothalli discoverable. It appears clear that very special conditions are necessary for the germination of the spores, conditions which are not of regular annual recurrence wherever *Phylloglossum* grows. Perhaps the most important of these conditions is the presence of a fungus with which the prothallium lives symbiotically. Such a symbiotic fungus has been found in the prothallia of all the species of *Lycopodium* in which the development is known.

Before describing the prothallia it will be well to state that they vary remarkably in external form. Such variations as depend upon the stage of development present no difficulty, but there are other differences which are probably accidental, being due to obstacles in the soil or to the depth beneath the surface at which the prothallium commenced its development.

One of the simplest and perhaps the youngest observed, consisted of an oval tuber below, from which rose a simple cylindrical shaft with rounded apex. Such a prothallium appears to closely correspond with the oldest prothallium of *L. cernuum* obtained in laboratory cultures by Treub. We may take the tuber, which is of constant occurrence, to correspond to the primary tubercle seen in the prothallium of *L. cernuum*.

A more advanced prothallium shows the cylindrical part of greater length and thickness, and its end slightly expanded into a crown, on which the first sexual organs appear. A little below the crown the tissues of the cylindrical body are conspicuously meristematic, especially on one side. This meristem lies below the archegonia, and its formation appears to be anticipatory of the descent of the embryo.

Older prothallia in which an embryo is already present are much more irregular in form. The crown, which may be conical, rounded, or projecting to one side, and then often shaped like the head of a horse, is commonly separated by a slight constriction from the much

enlarged part of the body of the prothallium. This latter part bears the embryo on one side; it is evidently formed by the increase of assimilatory tissues for the nutrition of the embryo. Below this swollen part the body contracts again to a cylindrical shaft, which passes downwards, to swell out again and terminate in the primary tubercle. It is this shaft which varies most; it may be long and narrow, straight or curved, or it may be shorter and stouter, or, occasionally, perhaps when the primary tubercle has been formed near the surface of the ground, it may be almost obsolete. The shorter, thick-set prothallia may be less than 2 mm. long; others may range up to thrice this, according to the length of the shaft. Rhizoids are produced in considerable numbers from the lower part of the prothallium, more especially from the tubercle.

The whole of the upper part of the mature prothallium is green, except the archegonial necks, but the minute chloroplasts are most abundant in the part below the crown. The cells of the shaft may contain chloroplasts, but the green colour passes away as we follow it downwards into the soil. Sections of the prothallia show little internal differentiation of the tissues, certainly nothing which is comparable to that described in *L. complanatum* and *L. clavatum*. The cells of the tubercle appear of a rounded polygonal form. They show scanty protoplasmic contents, and appear partly exhausted. Those of the shaft are elongated, on the surface they are rectangular, in the centre they tend to become longer and more pointed. Starch is often abundantly present in the cells, especially of the central part. An endophytic fungus may be traced in the cells of the lower half of the prothallium. The hyphæ are exceedingly fine; they have been traced passing in through the rhizoids. Around the tubercle they often form a close felt, which may pass below into a strand, which suggests at first sight that the base of the tubercle passes into a root. The tubercle is commonly brownish on the surface, and the strand is a darker brown and almost opaque. But sections show that it consists of fungus hyphæ.

The prothallia are monœcious, and the archegonial necks are a conspicuous feature on the crown. On a young prothallium I have found two or three only, but on plants bearing an embryo there may be from ten to twenty. They appear to be formed in basipetal succession. In a young prothallium they may be found on the summit of the crown, but in older ones they seem to occupy a lateral position around about half the circumference. The neck of the archegonium projects from the surface of the crown as a hemisphere of colourless cells, usually in two tiers of four cells each. The venter, with the large oosphere, lies at a little depth below the surface. The antheridia can scarcely be said to show in surface views, as they lie sunk in the crown. Sections show that the an-

theridial cavity is elongated at right angles to the neighbouring surface. The cover cells form a single layer. The sexual organs would seem to resemble those of *L. cernuum* more closely than those of other species of *Lycopodium*. There are no multicellular paraphyses amongst the sexual organs as in *L. Selago* and *L. phlegmaria*, but on some parts of the crown the surface cells are slightly papillose.

The thickness of the tissues renders it impossible to follow the details of the early development of the embryo, except in microtome sections. But it seems clear from the stages which have so far been examined, that the development at first is much like that of the embryo in *L. cernuum*. The embryo grows obliquely downwards and outwards, the part near the archegonial venter is the foot, at the opposite end are formed the stem-apex and leaf. The first part of the embryo to appear outside the prothallium is the tip of the leaf; it breaks out at a point on the side of the thicker part of the prothallium, below the crown; a fissure extends thence down the side of the prothallium, and the embryo appears as a short, cylindrical body, bluntly pointed at both ends, placed vertically, and still connected with the prothallium by the foot, which now has a lateral position. The ends of the embryo grow downwards and upwards respectively, and at a later stage what is really the apex of the stem appears inside the lower part of the embryo—that is, the embryo immediately on escaping from the prothallium forms a protocorm, apparently in the same manner that the adult plant forms its annual tuber. The pedicel of the tuber elongates downwards until the latter is placed at a safe depth, about 3 mm., in the soil. In the meantime the leaf grows up and attains a height of 2 to 5 mm. above the ground. I have not hitherto seen any formation of root during the first year of growth, the sporophyte seemingly depending largely for its supply of moisture upon the prothallium, which sometimes retains its vitality even after its crown becomes injured by drought. But sometimes, at any rate, rhizoids may be developed on the pedicel and protocorm. The leaf becomes green even before it escapes from the prothallium, and as soon as it reaches a little above the soil stomata are formed, and air passes into the intercellular spaces, whilst a slender strand of tracheids appears in the centre. The first protophyll has in fact exactly the structure of a small leaf as produced in later years. The further development of the sporophyte appears to be slow. By carefully dissecting out the plants in the soil one can find the remains of tubers and roots produced in former years. In many cases the plant comes up a second and a third year with only a single leaf.

It should be mentioned here that Crié has stated that he sowed the spores of *Phylloglossum* and obtained a colourless prothallium like that of *Ophioglossum*. But his statements have not been accepted,

and recent writers, as Vines, Bower, Campbell, Goebel, Pritzel,* distinctly state that the development of *Phylloglossum* is not known. I have not had access to Crié's original account, and Bertrand,† who quotes Crié's statements, was unable to obtain a germination of the spores during six years' experiments. But in any case it is clear that Crié's account was incomplete, for the prothallium becomes green, and even vividly green. According to Bertrand, Crié can only have seen the tubercle which precedes the prothallium proper.

It is not improbable that the prothallium may start life as a saprophyte, aided by the endophytic fungus, and I have found a young prothallium which was quite colourless save for a faint yellow tinge at the upper end, as well as two others, still without sexual organs, which showed only scanty chloroplasts. It is quite probable that on the germination of the spore the tubercle is first formed, and when this is at too great a depth in the soil to receive any light, it will doubtless be colourless. But I have never observed any fully developed prothallium that was not green above, whilst all prothallia which had succeeded in producing an embryo had reached the surface and attained a considerable development of chlorophyll.

A comparison of the prothallium of *Phylloglossum* with those of the few species of *Lycopodium* in which the gametophyte is known, shows that it is distinctly of a Lycopod type. But, as is well known, there is a remarkable diversity amongst the prothallia of the different species of *Lycopodium*. On the whole the prothallium of *Phylloglossum* probably resembles a prothallium of the *L. cernuum* type more closely than any other, though it is quite without the leaf-like assimilatory lobes of *L. cernuum*. Perhaps we are justified in regarding it as the simplest known type amongst the Isosporous Lycopodiinæ.

The general simplicity of the structure of the prothallium of *Phylloglossum* seems to favour the view that it is a primitive form of Lycopod. It is of course recognised that *Phylloglossum* is a permanently embryonic form, but the simplicity of structure of the mature sporophyte does not necessarily prove that it is a primitive form of the Lycopodiaceous phylum. Bower has expressed the view that *Phylloglossum* is probably a reduced form, and the absence of transitions between the simple cylindrical pointed protophylls and the scale-like sporophylls so like those of some species of *Lycopodium* may favour this view, if we regard these structures as homologous. Some observations which appear to be new may throw some light upon this question. Bower states that *Phylloglossum* has been seen branched. I am able to say that branching occurs in at least two distinct ways :—

* E.g., Goebel, 'Organographie der Pflanzen, 2te Teil, 1930,' p. 439. Pritzel in Engler and Prantl 'Die Natürlichen Pflanzen-familien,' Lief. 205 (1900), p. 575.

† 'Archives Botaniques du Nord de la France,' 1886, p. 221.

1. The spike or strobilus occasionally branches ; perhaps one strobilus in two thousand will be found forked, the two divisions becoming equally developed. I am of course only speaking of the form which grows in New Zealand, and this may possibly be a slightly more robust form than that found in Australia. The branching always takes place above the lowest sporophyll, sometimes quite at the base of the spike, near the lowest leaf, sometimes further up, or even close to the apex of the strobilus.

But even when the strobilus forks there is no transition of form between the sporophyll and protophyll. I have occasionally observed on the peduncle a leaf some distance below the rest of the strobilus, but such a leaf has always been of the sporophyll type. In the Australian form, investigated respectively by Bower and Bertrand, to whom we are indebted for most of our knowledge of *Phylloglossum*, eight was the largest number of protophylls found on a plant, whilst Bertrand urges on anatomical grounds that six is the normal number of protophylls. I have collected plants with twenty protophylls, whilst others with ten to fifteen such leaves are of common occurrence. But even in plants richest in protophylls no transition occurs between protophylls and sporophylls. So far as any evidence here available goes, it would almost seem as if the two structures were not strictly homologous.

To express my meaning in the language of a modern theory—the protophylls may have arisen from the differentiation of the lower region of a sporogonium (or the homologue of a sporogonium) in which this region had already acquired sterilised tissues, whilst the sporophylls arose from the upper fertile region of the sporogonium. If so, the protophylls cannot be regarded as sterilised sporophylls.

There appears to be no necessary connection between the number of protophylls and the reproduction by spores. Plants with two protophylls only may produce a weak spike, whilst plants of twenty protophylls may be barren.

2. In barren plants the new tuber is formed by the lowering of the apex of the stem, but in fertile plants a new outgrowth is formed, which Bower regards as adventitious. This may doubtless be considered as a form of branching. Neither Bertrand nor Bower observed more than a single new tuber formed in the examples at their disposal. Bower, indeed, was inclined to infer that as no other mode of vegetative reproduction was known, the plant depended for its multiplication solely upon the germination of the spores. But I have found that the formation of two new tubers is quite a common occurrence, though plants which form a single tuber are still in the majority. The two new tubers may be formed on opposite sides of the plant, in which case a slight dispersion of the plants takes place. Sometimes the two tubers are close together. Apparently they may be formed almost simul-

taneously, or in succession. Naturally, it is the stronger plants which most frequently multiply thus, but plants of a smaller number of protophylls may branch in this way. One plant of a single protophyll was found with two tubers forming.

The occasional occurrence of branching in the strobilus might be interpreted as an indication that the ancestors of the plant were once more abundantly branched. But it would be possible to take the opposite view that such branching is a nascent feature, that it is a new feature in the phylogeny. Bertrand regarded *Phylloglossum* as a form reduced on account of its semi-aquatic mode of life. But it is necessary to point out that *Phylloglossum* is not a semi-aquatic; Bertrand never had the advantage of seeing the plant in its native home. *Phylloglossum*, it is true, being a very small plant, can only grow whilst the surface soil is fairly moist, hence it forms a tuber and rests during the dry season. So far as I have seen, the plant grows rather better on a hill-top; or, at any rate, it grows there at least as well as it does lower down on the slope, and I have never found it in an actual swamp. It grows well on a slope where water can never lodge. Its roots spread rather horizontally, and seldom far downwards in the ground, as though it objected to a waterlogged soil.

Whilst it is possible that evidence may yet be adduced that *Phylloglossum* in some measure owes its simplicity to reduction, there appears to be little evidence for this at present. On the other hand, it may yet prove that *Phylloglossum* is an exceedingly primitive plant, possibly the most primitive of existing Pteridophytes. We have an explanation ready to hand of this exceptional retention of ancient characters, namely, the annual renewal of the embryonic stage in the formation of the protocorm. But however this may be decided, the relatively simple character of the gametophyte and the comparison of the mature sporophyte with the embryo of *Lycopodium cernuum* are in favour of the view that *Phylloglossum* is the most primitive of existing Lycopodiinæ.

“Notes on the Development of *Paludina vivipara*, with Special Reference to the Urinogenital Organs and Theories of Gasteropod Torsion. (Preliminary Note.)” By ISABELLA M. DRUMMOND. Communicated by Professor W. F. R. WELDON, F.R.S. Received November 26,—Read December 5, 1901.

I. The Urinogenital Organs.

Von Erlanger, in his work on the development of *Paludina*, made known for the first time the existence, at an early stage of development, of a rudimentary kidney belonging to the original left side of

the body. He describes it as formed in the same manner as the definitive kidney, that is, as an evagination of the pericardial wall. According to him, at a fairly early stage it degenerates, together with the ingrowth of the mantle cavity, which formed its rudimentary duct, and a later evagination of the pericardial wall in an almost identical position gives rise to the gonad. This soon loses its connection with the pericardium and becomes vesicular, while an ingrowth of the mantle cavity, presumably the arrested kidney duct, grows towards it and finally fuses with it to form the gonaduct.

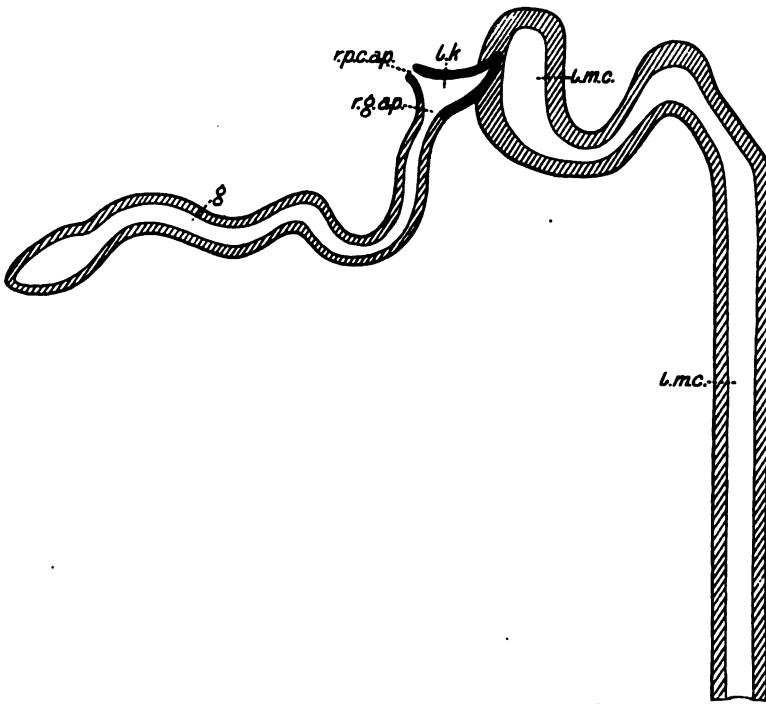
With the earliest stages of development as thus described the present researches are in complete harmony. At a time, however, when, according to von Erlanger, the rudimentary left kidney should have completely disappeared, it is found to be still present, and to show the normal relations both to the pericardium and the duct. No tendency to retrogressive development could be found in either the left kidney itself or in its duct. The development of the gonad also is not what von Erlanger describes it to be, for, whereas the kidney is an evagination of the original ventral wall of the pericardium, the gonad arises as a solid proliferation of the dorsal wall, and is connected with the kidney by a thickened ridge of pericardial epithelium. The originally solid gonad becomes secondarily hollowed out, and its lumen communicates with that of the kidney by means of this thickening, which also acquires a lumen.

In the adult, therefore, the genital organs may be considered as consisting of four distinct parts: (1) the gonad itself; (2) a specialised portion of the coelom represented by the lumen of the pericardial ridge; (3) the kidney of the adult right side, through which the genital products must pass to reach (4) the ureter of the definitive right side, transformed into a gonaduct. These parts are shown in the figure, which represents a longitudinal section through the genital organs before maturity is reached. The long tubular gonad is seen at *g*. The second division is indistinguishable at so late a stage, but must be represented by the proximal portion of the gonad, and opens into the kidney (*l. k*) at *r. g. ap.* The reno-pericardial aperture (*r. p. c. ap.*) is still open, but will close before maturity. The duct is seen at *l. m. c.*

II. Theories of Torsion.

There are two main schools of thought which have to be considered—that of which Bütschli and, more recently, Plate are the chief exponents, and that which is upheld by Pelseneer, Amaudrut, and Boutan. Whereas the former uphold that unequal growth of the left side of the body is an efficient ontogenetic cause of the torsion of the Gastropoda, the latter school believe that an actual twist of the body upon the head through an angle of 180° takes place in the course of development.

A renewed investigation of the development of *Paludina* shows that Bütschli's interpretation of it is open to much criticism. His point concerning the actual distance between mouth and anus remaining always the same is valueless, unless the mantle cavity be regarded as entirely formed by invagination. Even on the latter supposition rapid development of the original right division of the mantle cavity, at a time when torsion is being produced, shows that there is actually great activity in Bütschli's zone of cessation of growth.



Plate's suggestion that the liver, by its rapid growth on one side, alone brings about the asymmetry of the whole body is unsatisfactory, in that while torsion goes forward most rapidly the liver is almost stationary in its growth, while its chief development takes place after torsion is complete.

More fundamental than the above is the change in the position of organs which takes place through ontogeny. Bütschli and Plate have no explanation to offer of any displacement of organs in a vertical direction. A careful study of successive stages, however, reveals an orderly rotation of all the organs, whereby those originally dorsal become ventral, and *vice versa*. This can probably only be explained

on some such view as that put forward by Pelseneer, and this is further confirmed by evidence, during the stages in which torsion is being effected, of an actual twist of the oesophagus, which keeps pace exactly with the torsion of the whole body.

Various monstrosities were found, which all lent support to Pelseneer's hypothesis rather than to Bütschli's. Chief of these was a well-developed untwisted form, with symmetrical mantle cavity and kidneys, and a strong tendency towards exogastric coiling of the visceral hump.

With regard to the ontogenetic cause of torsion merely negative evidence was found. That it is almost certainly not due to antagonism of growth between the foot and the visceral hump is shown by a comparison between normal forms and monstrosities.

"On the Intimate Structure of Crystals. Part V.—Cubic Crystals with Octahedral Cleavage." By W. J. SOLLAS, D.Sc., LL.D., F.R.S., Professor of Geology in the University of Oxford. Received April 10,—Read May 23, 1901.

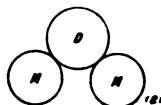
During the three years that have elapsed since the last part of this contribution was communicated to the Society, continued reflection has served only to confirm my belief that it is to the molecular volumes of crystalline matter we must turn for insight into its structure. There are several points of detail in which the results already obtained might be usefully modified, but the discussion of these may safely be postponed for the present, while we pass on to more important matters. For we have now reached a critical point in our enquiry, we propose to investigate more complex compounds than those hitherto considered, compounds also distinguished by a different crystalline structure. If we find, and I think we shall, that the constituent atoms of these compounds retain the specific volumes, which were determined from a study of very different cases, then our hypothesis will begin to appear less speculative, and we shall be able with greater confidence to extend our enquiries in other directions.

Triatomic compounds of the type H_2O are frequently represented graphically in the way shown by fig. 1. Molecules in which the atoms

FIG. 1.



FIG. 2.



M, Monad; *D*, Dyad atom.

are thus related cannot readily be arranged so as to give a stable structure, which will satisfy the conditions of cubic symmetry.

Let us then regard the two atoms, which form a pair, and which we may speak of as the paired atoms, to be approximated to each other as in fig. 2, and next let one such molecule be placed over

FIG. 3.



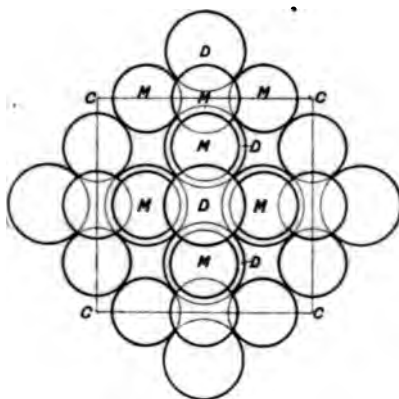
another similar one, but inverted, so that the paired atoms form the corners of a square, and the unpaired atoms rest upon them immediately over the centre of the square (fig. 3). The resulting figure is that of an octahedron, and might be regarded as the crystalline element from which other forms could be built up. Within certain limits determined by the relative dimensions of the paired and unpaired atoms, such a primitive octahedron might possess the "regular" character, *i.e.*, lines obtained by joining the centres of the atoms, or by drawing common tangent planes to their surfaces taken in threes, would be that of a regular octahedron. This would obviously be the case if the atoms were all of equal size, but it might also be if the unpaired atoms were larger than the paired atoms, but more closely approximate. Yet such an octahedron could not by itself be regarded as satisfying the requirements of cubic symmetry, for of the three rectangular axes which may be imagined to be drawn from the centre of the figure through the centres of the atoms, one, that which passes through the unpaired atoms, differs in its properties from the rest, inasmuch as it passes through two atoms of different kind to the other four.

It might be possible to uphold the view that in a chance distribution of such octahedra as many might be found with their single axis on one crystallographic axis as on another, but few crystallographers, I presume, would be prepared to admit that the exigencies of cubic symmetry could be so easily satisfied.

It is possible, however, to arrange our octahedra into groups which are completely symmetrical. In the case of regular octahedra, there would appear to be but one way of doing this. We may imagine three rectangular axes, the "tetragonal" axes of the cubic system: on each of these a primitive octahedron may be supposed to be placed, so that the axis passing through the unpaired atoms may coincide with one of the semi-axes of the rectangular system; and the other

two axes, passing through the paired atoms, lie opposite and parallel to the other two axes of the rectangular system, *i.e.*, to those on which the octahedra are not situate. If now the octahedra be pushed towards each other till they meet a form will result such as is shown in plan in fig. 4. It will be observed that this configuration is stable, for

FIG. 4.

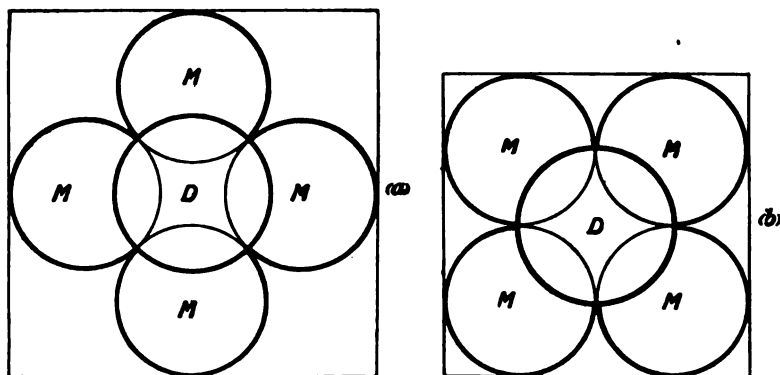


six of the unpaired atoms are in contact about the origin, the paired atoms are in contact with the unpaired and with each other, and further the paired atoms of adjacent octahedra touch each other in twos at each corner of the square figure which they form. This arrangement, in its unmodified form, does not appear to be a common one, but it is probably presented by fluorspar; we may term it for reference, Case I.

A second arrangement may be conceived as the result of rotating each of the primitive octahedra of the preceding form through an angle of 45° around the axis on which they lie; the side, and not the diagonal, of the square figure formed by the paired atoms will then lie parallel and opposite to the remaining rectangular axes (fig. 5 (b)). In this case if the primitive elements were regular octahedra, the paired atoms of adjacent octahedra would no longer touch, and the system would be unstable. Contact, however, may be brought about by pushing the unpaired atoms which lie away from the origin closer towards those which lie near it; this will bring about a divergence of the paired atoms of each octahedron from each other, and an approximation to their neighbours, till the latter are touched. With the establishment of contact stability is ensured, for although the paired atoms of each octahedron are no longer in contact with each other, they are supported on both sides by the unpaired atoms. I am unable to give an instance of this arrangement as occurring in fact. We may speak of it as Case II.

The two cases we have described are extremes between which a whole series of intermediate stages may exist. It may be observed, however, that gyrohedral hemihedry is associated with all these intermediate stages, and that none but the extreme cases afford complete holohedry.

FIG. 5.



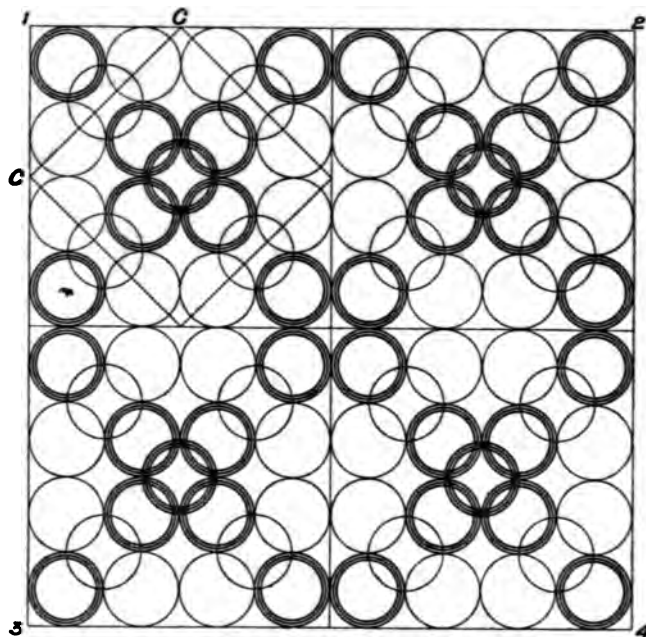
The configuration in the intermediate forms is dependent first on the relative dimensions of the two kinds of atoms, second on the distance between the unpaired atoms and the correlated distance between the paired atoms, and third on the amount of twist or rotation which must be given to the primitive octahedra to bring the paired atoms of adjacent octahedra into contact.

If the parameters on the axis of the unpaired atoms be shorter than those on the axes of the paired atoms, the first case is impossible, because when the unpaired atoms near the origin are in contact, the paired atoms would not have room to take up the position described in Case I, and would either have to part company with the unpaired atoms, or the square figure which they form would have to rotate about the rectangular axis on which its centre lies, till its diagonal when projected on a plane containing this axis and one of the others has the same length as the parameters of the unpaired atoms, *i.e.*, till the supposed octahedra project into principal sections of a regular octahedron.

To bring the various figures which may result from the different grouping of primitive octahedra to the test offered by a study of molecular volumes, it becomes necessary to devise a mode of partitioning space within the crystalline edifice in such a manner as to assign to each molecule its appropriate share. Let planes be drawn parallel to any two of the rectangular axes through the centre of each primitive octahedron, these planes will intersect to form a cube (*fig. 6, c.c.*), within which will be contained six molecules of the crystal, *i.e.*, six

halves of the primitive octahedra, and so much of interstitial space as must be assigned to them in an equal partitioning of space. The volume of this cube will therefore be given by dividing the molecular weight of the crystalline substance by its specific gravity and multiplying the molecular volume so obtained by 6. The cube-root of this number will give the edge of the cube.

FIG. 6



Plan of assemblage of primitive octahedra dyad atoms indicated by three concentric circles, monad atoms by single circles.

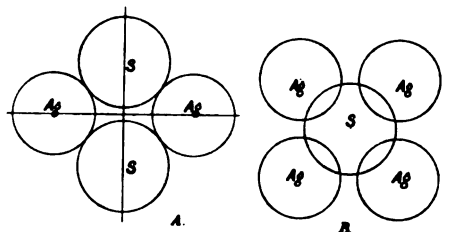
The mineral chosen for our first essay is silver sulphide (Ag_2S), because we have already obtained what appear to be very trustworthy values for the diameter of the atom of silver, which is 2.172, both in the free state and in its haloid compounds, and for sulphur, which in galena has a value of 2.408. The molecular weight of silver sulphide is 247.936, its specific gravity has been variously determined as from 7.27 to 7.32, we take the mean, which is 7.285, the molecular volume is thus 34.03, this multiplied by 6 is 204.18; extracting the cube root we have 5.889 as the length of the edge of the cube, to which all subsequent calculations must be referred.

Given the dimensions of the atoms of silver and sulphur as stated above, then if they are arranged as in Case I, the edge of the cube

which 6 molecules would occupy must have a length of 6.318, or 0.429 in excess of that obtained from the molecular volume. Silver sulphide consequently does not possess the structure described in Case I, and Case II may be shown to be impossible, unless the dimensions of the constituent atoms be taken as 2.192 for the diameter of silver, *i.e.*, 0.02 in excess of our previous determinations, and that of sulphur as 2.434, which is 0.03 in excess, and in this calculation the edge of the cube is determined from the maximum specific gravity instead of the mean.

It is of course possible that atomic volumes actually do vary to the extent this would indicate, but there would seem to be no apparent reason why silver, which retains with remarkable constancy the same atomic volume in the free state and in combination with the haloid elements, should undergo an increase in dimensions on combination with sulphur: it would therefore seem likely that the arrangement of the atoms in silver sulphide is gyrohedral. It has been asserted that the edge of the cube obtained when the atoms of silver sulphide are arranged according to Case I, measures 6.318, while that of our cube of reference obtained from the molecular volume is only 5.89. To transform the larger cube into the smaller, we may first push in towards the centre of the cube the outer atoms of sulphur (fig. 7, A), till

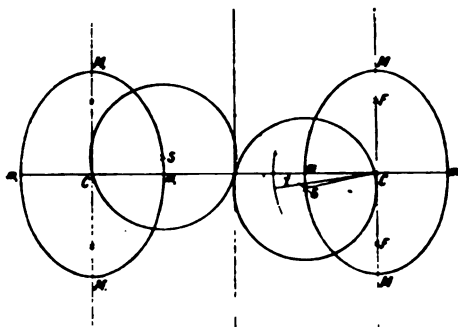
FIG. 7.



the point midway between them and the inner atoms lies on the centre of the face of the cube of reference. In this way we shall have reduced our deduced cube to the dimensions of that obtained from observation. This operation also fixes for us the relative position of the sulphur atoms. But by thus causing the atoms of sulphur to approach, we have at the same time caused the atoms of silver to diverge from each other in the manner shown in fig. 7, B, which is a section taken through two rectangular axes of a primitive octahedron. The atoms of silver are now much too wide apart to permit of the diagonal of the square figure which they form being brought into coincidence with a line drawn parallel and equal to the edge of the cube of reference. The requisite space for them on the face of the cube must be found by rotating the primitive octahedra, till the paired atoms at the corners

are brought just into contact. Considering the want of exactitude in our knowledge of the specific gravity of silver sulphide, the requisite amount of rotation may be obtained by construction, the errors attendant on this method lying within the errors of observation. Let

FIG. 9.



the centres of two octahedral elements be projected on a plane, which cuts the two rectangular axes on which they lie at 45° , and is parallel to the third, i.e., parallel to two of the diagonals of the cube of reference. Then let each octahedron be supposed to rotate through a complete circle, the centres of the paired atoms will then project into an ellipse which may be used as a kind of directrix. The spheres representing the paired atoms will always project as circles. Find the point on the ellipse at which the centres of two circles representing the silver atoms must be situated so as just to touch. The angle which a line joining one of these points to the centre of the octahedron makes with that joining the centres of the two octahedra is that sought. In the case of silver sulphide it amounts to $21^\circ 52'$.

Thus, while preserving the dimensions we had previously obtained for the atoms of silver and sulphur, we arrive at a structure which completely explains the cleavage of silver sulphide, and satisfies the conditions of symmetry imposed by the system to which it belongs.

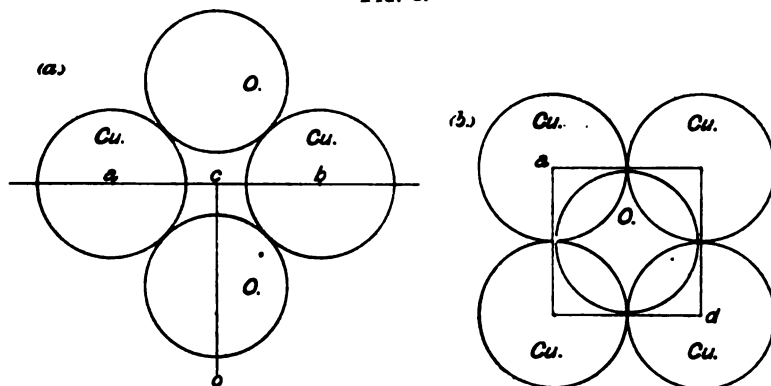
Gyrodredry has not yet been described in silver sulphide, but it has been discovered in cuprite by Professor Miers. To this mineral we turn therefore with particular interest.

The molecular weight of cuprite, Cu_2O , is 142.6, its specific gravity has been determined with most discordant results; we select that given in 'Watts' Dictionary of Chemistry,' viz., 5.749 at 4°C . The molecular volume obtained from this is 24.8043, and the edge of the cube of reference measures 5.3. The diameter of an atom of copper in the metal itself has been previously determined as 1.918, that of oxygen on a somewhat doubtful basis, as 1.851.

Our want of certitude regarding the precise specific gravity of

cuprite, justifies the treatment of the ensuing discussion to a great extent by construction. Let ab (fig. 8 (a)) be any line drawn in the face

FIG. 8.



of the cube of reference through its centre; let co be a tetragonal axis of the cube on which, by the conditions of the case, an atom of oxygen must be situated at a distance of 1.31 from the centre of the cube, and 1.34 from c , the centre of the face. To obtain a section through a primitive octahedron of which the line ab shall be an axis, circles may now be drawn 1.92 in diameter, representing sections through atoms of copper; their centres must be situated on ab and at such a distance from c , that the circles just touch that representing an atom of oxygen.

Using ab as a diagonal, construct upon it a square (fig. 8 (b)) ad , and at the corners of the square as centres describe circles of the given diameter to represent atoms of copper, this will give us a section through the primitive octahedron containing the axes on which the four atoms of copper are situate. We may sometimes speak of this as the copper square.

When the construction is made, it will be found that the circles intersect to a very slight but appreciable extent. The smallness of the amount of the intersection suggests that we have not been far out in our calculation, and leads to the conclusion that some slight reduction in the dimensions of the atoms of copper accompanies a change from the free state to that of combination, as indeed might naturally be expected when the loss of energy which accompanies this change is considered. On this assumption we may legitimately reduce the diameters of our circles in fig. 8b till the latter just touch. The diameters thus pass from 1.918 to 1.90. This is a limiting value; the atoms may be slightly smaller, for they are probably not in actual contact, but they cannot be larger.

The effect of the reduction in the diameters of the atoms of copper

must next be represented in the section shown in fig. 8a. The correction is readily made by a slight enlargement of the atomic volume of oxygen, the diameter of this becoming 1.89.

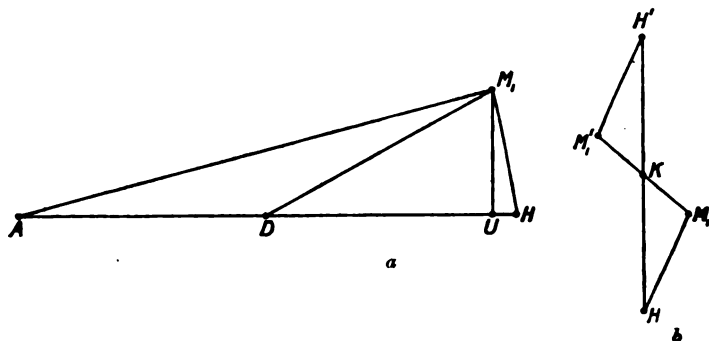
We are now in a position to determine the amount of rotation which the primitive octahedra must undergo so that the centres of the atoms of copper may be situated in the face of the cube of reference.

Mr. Harold Hilton, Fellow of Magdalen College, has kindly supplied me with the following solution, in place of my geometrical construction.

Let A (fig. 10, a) be the centre of the group of the six octahedra, each of which consists of four copper and two oxygen spheres. Let D be the centre of one of the inner oxygen spheres, $M_1M_2M_3M_4$ the centres of the four copper spheres in the same octahedron; and let D' , M' correspond to D, M in a neighbouring octahedron. Let the centre of the square $M_1M_2M_3M_4$ be U. Let the radii of the copper and oxygen spheres be r_1 , r_2 respectively.

The centres of all the copper spheres of the six octahedra lie on a sphere with centre A. Let AD, AD' meet this sphere in H, H' (fig. 10, b), and let the great circles M_1M_1' HH' meet in K; M_1 , M_1' being the centres of two copper spheres (from different octahedra) which touch.

FIG. 10.



Then we have from symmetry

$$HK = \frac{1}{2}HH', \quad M_1K = \frac{1}{2}M_1M_1'.$$

And hence readily

$$\left. \begin{aligned} HK &= \frac{\tau}{4} \\ \sin KM_1 &= \frac{r_1}{AM_1} \\ \sin HM_1 &= \frac{M_1U}{AM_1} = \frac{\sqrt{2}r_1}{AM_1} \end{aligned} \right\} (z).$$

The angle KHM_1 is the angle of rotation required, and from the spherical triangle KHM_1 we have

$$\begin{aligned}\cos KHM_1 &= \frac{\cos M_1K - \cos HK \cdot \cos HM_1}{\sin HK \cdot \sin HM_1} \\ &= \frac{\sqrt{2AM_1^2 - 2r_1^2} - \sqrt{AM_1^2 - 2r_1^2}}{\sqrt{2}r_1}\end{aligned}$$

(substituting from (a) and reducing); where

$$\begin{aligned}AM_1^2 &= AU^2 + M_1U^2 = (AD + \sqrt{DM_1^2 - M_1U^2})^2 + M_1U^2 \\ &= 2r_2^2 + (r_1 + r_2)^2 + 2\sqrt{2}r_2\sqrt{(r_1 + r_2)^2 - 2r_1^2};\end{aligned}$$

$$\text{for } AD = \sqrt{2}r_2, \quad M_1D = r_1 + r_2, \quad M_1U = \sqrt{2}r_1.$$

(This method may be readily extended to groups of spheres, in which two of radius r_2 are each in contact with g spheres of radius r_1 , whose centres form a regular polygon; and s spheres of radius r_2 are placed in contact to form a regular polygon of s solid angles.)

For the arrangement to be possible, we must have $DU \leq r_2$, i.e., $\sqrt{(r_1 + r_2)^2 - 2r_1^2} \leq r_2$, i.e., $r_2 \leq \frac{1}{2}r_1$.

It is of interest to consider the possible effects of change of temperature upon the volume of such a configuration as we have described. In discussing this subject in the case of silver iodide, it was suggested that the volume of the atoms of iodine possessed a higher coefficient of expansion than that of the atoms of silver; from this analogy and that of sulphur and selenium as compared with copper, we may plausibly suppose that the oxygen of cuprite has a larger coefficient than the copper. If this view be admitted as having some likelihood, important consequences follow from it; to examine these let us consider the effects of a rise of temperature upon the configuration; the first result will be an expansion of atomic volumes, but since according to supposition, the atoms of oxygen enlarge at a higher rate than those of copper, the disparity in size of the two kinds of atoms will be increased, but if the centres of the oxygen atoms be regarded as fixed in space, this will bring about an increase in the divergence of the atoms of copper from another, and thus an increase in the length of the diagonal of the copper square. So far all these changes are in the direction of expansion, but it has next to be remembered that an increase in the length of the copper diagonal does not necessarily involve an increase in the total volume of the configuration, because this diagonal may rotate in a direction which leads towards the configuration of the limiting case 2, which is that of minimum volume.

Expressed crudely it may be said that room is furnished for the increased volume of the oxygen by utilising the space which lies in the

centre of each primitive octahedron in the first place, and next that which lies in the middle of the face of the complex octahedron.

The total change in volume of the crystal would thus depend primarily on the expansion of the atomic volumes, and next on the contraction which follows from a change of configuration; and it is from the supposed counteraction of these two opposing factors that we may find an explanation of the extraordinarily small coefficient of expansion which is characteristic of cuprite. Its value as measured by Fizeau is 0.00000093, that of sulphur (an homologue of oxygen) being 0.000064, and that of copper 0.0000169; copper in the free state thus possesses a coefficient eighteen times greater than that of cuprite, while that of sulphur is seventy times greater.

The variation in the coefficient of expansion was very carefully determined by Fizeau, and found to be 2.18. From this it follows that with increasing rise of temperature the effect of the expansion of atomic volumes gains upon that of the contraction due to change of configuration; while with fall of temperature the contrary is the case, and a point may be looked for at which further cooling will cease to be accompanied by contraction; at this point, determined as -4°C . by Fizeau, cuprite will attain its maximum density.

If the atoms of copper and oxygen are to remain in contact, so that the six atoms of oxygen about the centre of a crystalline element shall touch each other as well as the atoms of copper with which they are associated, then when the configuration is that of maximum volume, the atoms of oxygen cannot have a less diameter than the atoms of copper. We have already seen, however, that at 15°C . the atoms of oxygen do possess a slightly smaller diameter than that of copper. It is therefore evident that further cooling if it tend towards producing the configuration of maximum volume will effect a separation of the six central oxygen atoms from each other, and this, though it will not destroy, will certainly diminish the stability of the system; and consequently we may expect to find, if cooling be continued far below 15°C ., that the existing configuration will be overturned, and the compound will acquire the symmetry of another crystalline system.

The cleavage of cuprite, as observed by Professor Miers, is most perfect parallel to the faces of the cube; this may possibly be connected with the fact that these planes are situated at right angles to the tension which we infer to exist between oxygen and copper along the axes of the octahedron.

In concluding this account of cuprite attention may be directed to the interesting case of cupric oxide, which has been made the subject of frequent comment by students of atomic volumes, on account of the remarkable relation which seems to exist between the volume of oxygen as it exists in this compound, and in the lower oxide which we

have just studied. Thus comparing Kopp's volumes in the two cases we have:—

Cuprite (Cu_2O)..... m.v., 24·77, deduct Cu_2 (14·1) = 10·67 (O).
 Tenorite (CuO)..... m.v., 12·67, „ Cu (7·05) = 5·62 (O).

Thus by Kopp's method the volume of oxygen in cuprous oxide is nearly twice that which it possesses in cupric oxide, and as this is far from being an isolated instance of this relation of the volumes of oxygen in related compounds, Kopp very naturally came to the conclusion that the volume of oxygen varies in different compounds by some multiple, and since he found it exhibiting a still less value in some other cases, he imagined it might be reduced to one-half or one-quarter of its normal value. In the present instance there is no need to have recourse to such an explanation. In the crystalline structure of cuprite open spaces exist, sufficiently large and numerous to contain one more oxygen atom than the molecule possesses; in this respect it might be said that the volume of oxygen in cuprite is double that it has in tennorite, but this would be a very illogical way of stating the facts; the vacant space is no more the property of the oxygen than of the copper, and, if our conclusions are correct, the truer view would be that in both oxides the volume of oxygen is identical, or very nearly so, and that the apparent difference in volume is a natural result of difference in configuration of the crystalline structure.

Fluorspar, CaF_2 : m.v. 78; sp. gr. 3·15 to 3·18, mean (taken) 3·165; m.v. 24·645. $\sqrt[3]{(\text{m.v.} \times 6)} = 5·288$, the edge of the cube of reference.

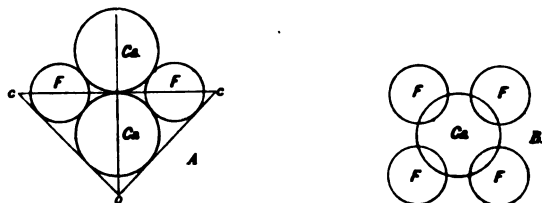
This mineral is of importance to our investigation, as it affords an opportunity of obtaining a value for the volume of fluorine; but we are confronted by a difficulty at the outset, since the diameter of the atom of calcium has been calculated from a single instance only, and that a not very promising one, viz., calcium oxide; it was concluded to be 2·27. If now we place an atom of calcium in octahedral contact with five others about the centre of the cube of reference, we shall find that it is too large to fit in, the length of a tetragonal axis that it would appropriate is 2·739, while the length on the axis from the centre to the surface of the cube is only 2·644; the atom would consequently protrude for a distance of 0·095, and the greatest possible diameter which it can possess to bring its surface just flush with the face of the cube is 2·19.

We may accept this provisionally as the approximate value for the diameter of an atom of calcium; it differs from that previously obtained by 0·08.

Since the outer surface of the atom of calcium contained within the cube is in contact with the face of the cube, it follows that it also touches the atom of calcium which lies outside the cube, and completes

the primitive octahedron in that direction. As a consequence the atoms of fluorine must be as widely divergent from each as possible, and since no gyrality has been detected in any of the numerous forms of fluorspar, it may be concluded that they are disposed in holohedral symmetry; this cannot be that of case 2, for this would not permit of contact between the fluorine atoms of adjacent primitive octahedra,

FIG. 11.



and thus would not be compatible with stability; we are thus led to attribute to fluorspar the structure of case 1. Calculating from these premises it is found that the diameter of the atom of fluorine is measured by the quantity 1.573; from this it follows that the gross volume is 3.892, a result in accordance with what knowledge we possess of the gross volume of fluorine in other compounds. It will be observed that this number is smaller than that previously assigned to the diameter of an atom of hydrogen.

The hardness and cleavage of fluorspar stand in direct connection with its structure; its perfect octahedral cleavage may be correlated with the vacant spaces which remain in the crystal as a consequence of the symmetrical disposition of the complex octahedral groups; its hardness both with this and the arrangement of the atoms within the octahedral groups. The difference in the hardness in different directions has been very carefully explored; on the faces of the cube its maximum lies parallel to the tetragonal axes, its minimum to the digonal axes, and it is in these directions that the atoms are respectively in closest proximity and the reverse; on the octahedral faces the maxima lie parallel to the trigonal axes, and the minima to the digonal axes, which again are directions of greater and less proximity of the atoms.

In concluding this part of the subject I desire to express my cordial thanks to Professor Miers for much friendly criticism and useful advice.

December 12, 1901.

Sir WILLIAM HUGGINS, K.C.B., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

- I. "On the Action of the Spurge (*Euphorbia hiberna*, L.) on Salmonoid Fishes." By Dr. H. M. KYLE. Communicated by Professor MCINTOSH, F.R.S.
- II. "Contributions to the Chemistry of Chlorophyll. No. VIII.—Changes undergone by Chlorophyll in passing through the Bodies of Animals." By Dr. E. SCHUNCK, F.R.S.
- III. "On the Constitution of Copper-Tin Alloys." By C. T. HEYCOCK F.R.S., and H. F. NEVILLE, F.R.S.
- IV. "The Effective Temperature of the Sun." By Dr. W. E. WILSON, F.R.S.

The Society adjourned over the Christmas Recess to Thursday, January 23, 1902.

"Contributions to the Chemistry of Chlorophyll. No. VIII.—Changes undergone by Chlorophyll in passing through the Bodies of Animals." By EDWARD SCHUNCK, F.R.S. Received November 29,—Read December 12, 1901.

Considering the large quantity of food containing chlorophyll consumed by animals feeding on green herbage, it seemed to me it might be of interest to ascertain whether any, and if so what, changes are undergone by the chlorophyll of the food during its passage through the animal body.

It is stated in works on physiological chemistry that the solid excrements of animals contain chlorophyll; but this statement refers apparently to human fæces, and is certainly not correct as regards those of herbivora if unchanged chlorophyll is meant. It would indeed, *à priori*, seem very improbable that chlorophyll, after exposure at a somewhat elevated temperature to acids and other agents, such as it would meet with in its passage through the animal system, would remain unchanged, though, on the other hand, chlorophyll products of decom-

position, some of which, as I have pointed out on former occasions, are very stable bodies, might be looked for in the excrements of animals.

Having treated some of the fæces of a cow that had been fed for some time on grass only, with boiling alcohol, I obtained a dark greenish-brown extract showing an acid reaction, a quantity of undigested matter consisting of stems, woody fibre, &c., being left undissolved. A little of the filtrate, on being mixed with water and shaken up with ether, gave a golden-yellow supernatant liquid, which, if chlorophyll had been present in the material used, would have shown a decided green colour, and would have exhibited the absorption spectrum of chlorophyll. In place of the latter, however, it showed the four absorption bands of phylloxanthin, as well marked, indeed, as I have ever seen them. This simple experiment proved conclusively the absence of chlorophyll, but in its place the presence of one of its products of decomposition. I have made no attempt to isolate and purify this supposed phylloxanthin, this being rendered difficult owing to the large admixture of impurities, which are chiefly of a fatty nature. The ethereal liquid just referred to would also have contained the urobilin of the fæces if present, so I imagine at least if I have rightly understood what has been stated regarding that body. The properties of urobilin, however, are not very marked. It is only by its absorption spectrum that it can with certainty be detected, but this spectrum is too inconspicuous and too faint to be easily seen in a solution containing at the same time phylloxanthin with its dark well-defined bands, which would completely mask those of urobilin situated as these are in the same region of the spectrum. Its presence, too, if proved, would have been of little interest from my point of view, and I therefore made no attempt to establish its presence or absence.

The extract of fæces with boiling alcohol gave, after filtration and cooling, a dark-coloured flocculent deposit, which was filtered off, dried, and treated with boiling chloroform. The filtered chloroformic liquid left on evaporation a quantity of purplish-blue lustrous crystals. These crystals, which are of considerable interest as regards both their properties and their origin, will be described presently. They are more readily prepared by extracting cow-dung after pressure between folds of paper with cold chloroform, filtering, and evaporating the filtrate slowly in a warm place, when the substance separates in the form of brilliant semi-metallic spangles floating in the liquid, which, after collecting and washing with alcohol, have the appearance of a lustrous crystalline mass. From the brilliancy of its appearance an observer might easily be deceived as regards its quantity, which is not really large. I have, however, obtained sufficient to enable me to determine its chief properties, and to justify the conclusion that it is a derivative of chlorophyll closely resembling, though not identical with, phyllocyanin, as I shall presently show.

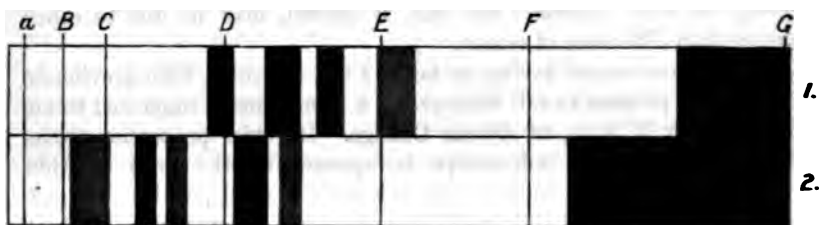
For such as wish to prepare this substance, I may state, as the result of my experience, that it can only be obtained from the fæces of cows or other herbivora that have been fed exclusively on grass or other green vegetable food; those of stall-fed cattle, nourished as they usually are to a great extent with oil-cake, yield none, on account perhaps of the large amount of fatty matter present in that case in the fæces. I may add that the solid excrements of sheep that have been at pasture for some time and living on herbage only, on being treated as above described, yield the same substance, but I have made no experiments with the fæces of other herbivora. I imagine that the yield of this peculiar substance is greatest from material collected in spring or early summer; but this, if correct, may be due to other causes than difference of season.

This substance not having, so far as I can ascertain, been previously observed, I propose to call *Scatocyanin*, a name kindly suggested to me by Professor Wilkins, of Owens College. Its chief properties are as follows:—Under the microscope it appears in the form of thin rhombic plates or elongated flat prismatic crystals, which are pale brown by transmitted light, of a purplish-blue colour with a brilliant metallic lustre by reflected light. When heated between watch-glasses it is decomposed without melting or swelling much or yielding any sublimate; heated further on platinum, it burns away, leaving a little ash. It is almost insoluble in boiling alcohol, ether, carbon disulphide, and benzol, but it dissolves, though not readily, in chloroform, giving a solution which shows an absorption spectrum of five bands, almost identical with those of phyllocyanin. It dissolves in boiling glacial acetic acid, giving a fine crimson solution, which, when sufficiently dilute, shows an absorption spectrum of four bands—of which the two first are well defined, the third faint with some obscuration between it and the second band, the fourth band also faint and not well defined (see fig.). From a saturated solution in boiling acetic acid the substance separates on cooling and standing in lustrous purplish-blue needles.

It dissolves in concentrated sulphuric acid with a brilliant grass-green colour, which, on standing, changes to purplish-blue. The solution now shows a characteristic absorption spectrum of five bands, of which the first and fifth are faint and poorly defined, the second and third well defined, but the fourth only moderately so, while there is much obscuration between the fourth and fifth bands, with just a trace of a sixth band in the green between the fifth band and the total obscuration (see fig.).

The sulphuric-acid solution on being mixed with several times its volume of water changes its colour from purplish-blue to a fine purple without giving any precipitate and without showing any change in its absorption spectrum. On standing, however, for some time the liquid

becomes nearly colourless, and deposits rosettes of lustrous crystalline needles, presumably of unchanged substance; so that it appears no sulphonic acid is formed in this reaction, the colour of the solution in sulphuric acid being due probably to a loose combination of the substance with the acid. The substance is soluble in boiling aniline; the solution is dull red, and gives with alcohol a deposit of brilliant needle-shaped crystals, the filtrate from which shows the same spectrum as the solution in acetic acid. The substance is insoluble in aqueous caustic potash, but dissolves in alcoholic potash, giving a yellow solution.



Absorption Spectra of Scatocyanin Solutions.

1. Scatocyanin in glacial acetic acid.
2. The same in concentrated sulphuric acid.*

There is another substance of definite character and assuming a crystalline form which I have obtained, though not always, along with the preceding. Having treated the fæces of a cow that had lived for some time on green vegetable food, with acidulated alcohol (seventeen parts of rectified spirit to three of sulphuric acid, the mixture prescribed for the preparation of so-called stercobilin), I added water to the filtered extract and shook up with chloroform. The chloroformic liquid after separation was evaporated, when it left a brown syrup. This, treated with boiling alcohol, dissolved in part, a semi-crystalline pink residue being left undissolved, which, after separation, was found to be soluble in boiling acetic acid with a crimson colour, and consisted doubtless of scatocyanin. A further quantity of this was deposited on again evaporating; on further evaporation, the filtrate left a thin brown syrup which was mixed with a large quantity of alcohol. On standing, a voluminous crystalline deposit separated, which was filtered off and slightly washed with alcohol.

The following are the chief properties of the substance so prepared:—When dry it has the appearance of a dull red mass of crystalline needles. Heated on platinum, it melts easily to a brown mass, solidifying again on cooling; on further heating, it gives off red fumes

* These spectra were mapped by my son, Charles A. Schunck.

and burns away, leaving no ash. When heated in a test-tube or between watch-glasses, it gives a small quantity of amorphous brown sublimate with a few crystalline needles. It dissolves in boiling alcohol, giving a bright yellowish-red solution, which, on cooling, deposits crystalline needles, becoming almost colourless. It is moderately soluble in ether, benzol, and glacial acetic acid; very soluble in chloroform. The chloroformic solution shows no absorption bands, only obscuration in the blue. It is quite insoluble in caustic potash liquor even on boiling. It dissolves in concentrated sulphuric acid, giving a dull red solution, which, after the addition of a considerable quantity of acid, shows no absorption bands, only obscuration in the blue and green; on the addition of water, the solution turns reddish-yellow without giving any precipitate, but on standing for a short time the colour turns to a fine violet, and then shows a broad ill-defined absorption band between the red and green. Not showing any very characteristic reaction but the one last mentioned, it must remain doubtful how and where it originates. It may possibly be a constituent of the green parts of plants not hitherto observed, though I believe it myself to be a derivative of chlorophyll—meaning by chlorophyll the *ensemble* of the colouring matters of green leaves—formed by some unknown process in the animal economy, but the fact of its solutions showing no absorption bands does not lend countenance to this view. Having also observed it on one occasion only, I do not feel justified in giving it a name or in placing it in any known category of vegetable or animal colouring matters.

Another constituent of the fæces remains to be mentioned. It was referred to above as showing in solution the absorption spectrum of phylloxanthin. After all the substances capable of assuming a crystalline form have been separated from the alcoholic extract of the fæces, this phylloxanthin-like substance is found in the final mother liquor. I have not succeeded in obtaining it in a crystalline or any other definite form, on account probably of the large quantity of fatty matter with which it is associated; but there is no reason, I think, to suppose that it differs essentially from the phylloxanthin described in previous communications as a product of the action of acids on chlorophyll.

The conclusions to which the experiments above described lead may be summarised as follows:—

1. The fæces of animals supplied with green vegetable food only—such at least as have so far been examined—contain no chlorophyll, but in its place substances which must be supposed to be derivatives of chlorophyll, formed partly by the action of acids on the chlorophyll of the food, partly by some agency to which the latter is subjected in its passage through the body.

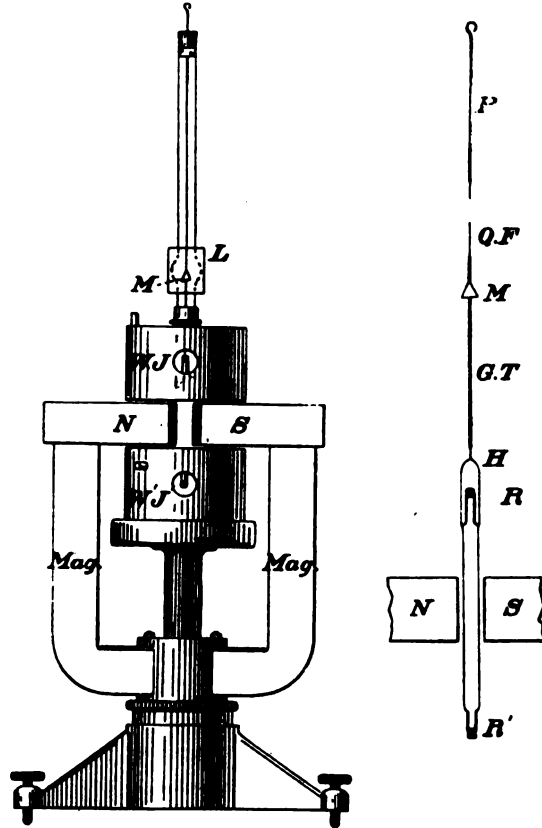
2. Of these substances, one seems to be identical with phylloxanthin,

a well-known product of decomposition of chlorophyll. Another is a substance of well-marked properties, nearly resembling, but not identical with, phyllocyanin. It has not, so far as my experience goes, been hitherto observed as a result of any process of decomposition to which chlorophyll has been subjected outside the animal body. I consider it as a body *sui generis*, characterised by its fine purplish-blue colour and its brilliant metallic lustre. The existence of other products in addition to these two is possible. On one occasion, indeed, a definite crystalline substance was obtained, which seemed to be peculiar, but that it was in any way connected with chlorophyll could not with certainty be maintained.

“The Effective Temperature of the Sun.” By W. E. WILSON,
D.Sc., F.R.S. Received December 5,—Read December 12, 1901.

In March, 1894, Dr. G. Johnstone Stoney communicated to the Society a memoir by myself and Mr. P. L. Gray, entitled “Experimental Investigations on the Effective Temperature of the Sun,” which was published in the ‘Phil. Trans.,’ A, vol. 185 (1894). In these investigations the method we adopted was as follows:—A beam of sunlight was sent horizontally into the laboratory by means of a Stoney single-mirror heliostat. The mirror was an optical plane of unsilvered glass, and the beam was directed into one aperture (A) of a differential Boys’ radio-micrometer. The other aperture (B) received the radiation from a strip of platinum, which could be raised to any desired temperature by an electric current supplied by a battery of accumulators. The temperature of this strip was at any moment determined by its linear expansion, the instrument being previously calibrated by melting on it minute fragments of AgCl and of pure gold, as in Joly’s maldometer. In front of the aperture (B) of the radio-micrometer was placed a stop with a circular hole of 5·57 mm., and the distance of this hole from the receiving surface of the thermo-couple was 60·2 mm. This gave for the angle subtended by a diameter of the aperture at the receiving surface $5^{\circ}30'1''$. Knowing then (i) the ratio which the angular diameter of this circular aperture bears to that of the sun, (ii) the temperature of the platinum strip at the moment that the radio-micrometer is balanced, (iii) the amount of the sun’s radiation lost by reflection from the heliostat mirror and also by absorption in the earth’s atmosphere, it is possible on any assumption with regard to the law connecting radiation with temperature, to determine the effective temperature of the sun. A series of very accordant observations were made in this way, the mean of which gave 6200° C. as the effective solar temperature.

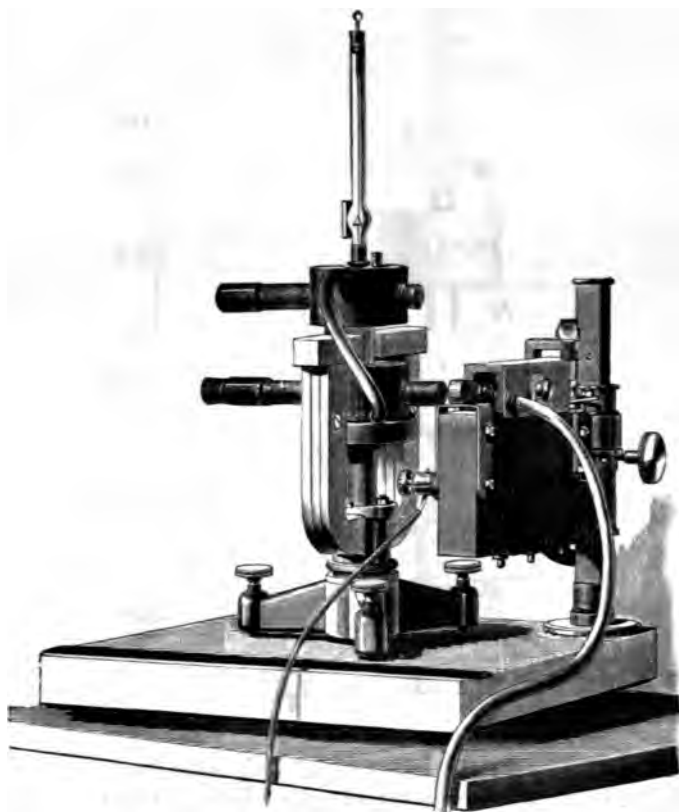
For the details of the apparatus and the complete method of reduction of the observations, the original memoir in the 'Phil. Trans.' may be referred to.



In order to protect the incandescent strip from draughts of air it was covered with a water-jacket of gilded brass. This was provided with a circular hole in one of its longer sides, through which its radiation could reach the aperture of the radio-micrometer. The internal walls of this water-jacket being highly polished, it has occurred to me, since the publication of the memoir referred to, that possibly some of the radiation from distant parts of the platinum strip may have been reflected backwards and forwards from the polished walls and the strip itself, ultimately escaping through the aperture and reaching the radio-micrometer, thus increasing the amount of radiation which should have reached it directly from the strip alone.

In order to test this surmise I first took a number of readings at known temperatures with the walls of the water-jacket polished as

before. I next smoked the surface of the walls well, and found that the amount of radiation coming from the aperture was then sensibly reduced. It is also possible that changes in the condition of the surface of the platinum strip may effect its emissivity, and in fact it is very doubtful whether it is possible to determine with any degree of accuracy what the emissivity of bright platinum is, relatively to lamp



black. In the original memoir we took Rosetti's estimate of 35 per cent. as the most probable value for this quantity, but as our former estimate of the solar temperature depends greatly on this factor, to which so much uncertainty attaches, I thought it would be a distinct advance to abolish entirely the platinum strip as a source of radiation, and to substitute in its place a uniformly heated enclosure which would radiate as an absolutely "black body."

In 1895 Mr. Lanchester pointed out to me that such an enclosure *would be a theoretically perfect radiator*; while Lummer, Paschen and others have shown that the law connecting temperature and

radiation from such an enclosure confirms in a remarkable manner Stefan's law of radiation, viz., $R = \sigma T^4$.* Since therefore the results of several independent investigations corroborate this law, I have felt justified in applying it to the results of my observations.

On consideration it seemed that the most convenient form of radiator would be a long tube closed at one end, and uniformly heated in a gas furnace. Accordingly a porcelain tube, 2 feet in length and 1 inch internal diameter, was fitted into a Fletcher gas-tube furnace. This was afterwards changed for an iron tube, which was employed in the observations on September 30th, given below.

A plug of asbestos was inserted in the tube at about 10 inches from the end farthest from the radio-micrometer, and resting against this plug was the end of a Callendar platinum-resistance thermometer. This was connected with one of Professor Callendar's electric recorders, so that during an experiment the temperature of the tube was registered continuously on the paper wrapped round the drum of the instrument. In front of the open end of the tube, and between it and the radio-micrometer, was placed a large brass water-screen, through which a copious supply of water passed. In front of the aperture (B) of the radio-micrometer this screen was provided with a rectangular aperture. One side of this aperture was formed by a slide moved by a micrometer screw reading to 0.01 mm. By this means the area of this aperture at any time could be measured with precision. Its fixed sides were 5 mm. apart, and as the movable side had a range of 5 mm., the maximum area of the aperture was 25 sq. mm. The distance (d) of this aperture from the surface of the thermo couple was 66.3 mm.

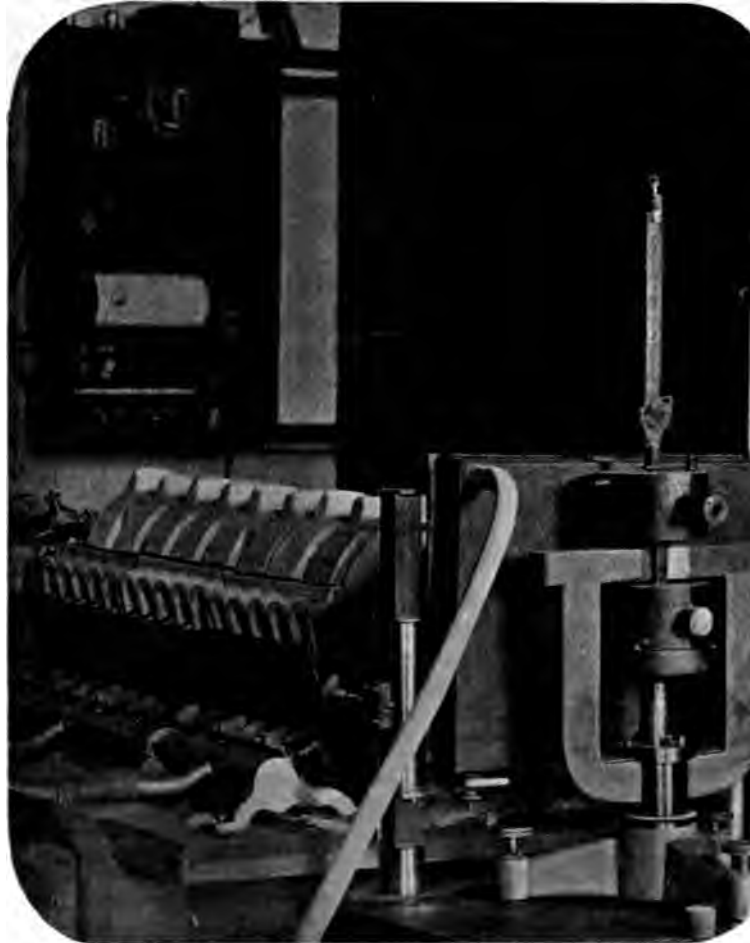
To make an observation the tube was heated to as high a temperature as the furnace was capable of, and when a steady temperature had been obtained, the amount of radiant heat coming from the interior of the hot tube and passing into the aperture (B) of the radio-micrometer was adjusted by the micrometer screw until a balance was obtained with the radiation coming from the Sun through the aperture (A).

If ρ is the angular semi-diameter of the sun, its radiation outside of our atmosphere is $K \cdot \pi \sin^2 \rho$, where K is a constant depending on the sun's temperature.

Again, if α be the height of the slit through which the radiation from the hot tube reaches the radio-micrometer, and β its width, the radiation may with sufficient accuracy be expressed by $K\alpha\beta/d^2$. Assuming Stefan's law, the radiation of the sun outside our atmosphere is $\sigma\theta^4\pi \sin^2 \rho$, θ being the effective temperature of the sun.

* Stefan, 'Sitzber. d. k. Akad. zu Wien,' vol. 79, 1879 (Abth. 2), p. 391; Boltzmann, 'Wied. Ann.,' vol. 22, 1884; M. Planck, 'Drude Ann.,' vol. 1, No. 1, 1900; Paschen, 'Wied. Ann.,' vols. 58, 60, 1896, 1897; Lummer and Pringsheim, 'Wied. Ann.,' vol. 63, 395, 1897.

The percentage transmitted is p , therefore the radiation before reflection from the heliostat is $\frac{ap\theta^4}{100} \pi \sin^2 \rho$.



At reflection q per cent. is transmitted, therefore the radiation received by the radio-micrometer is

$$a \cdot \frac{pq\theta^4}{(100)^2} \cdot \pi \sin^2 \rho \quad \dots\dots\dots ($$

Also the radiation received from the hot tube is

$$a' (T^4 - T_0^4) \cdot \frac{\alpha\beta}{r^2} \quad \dots\dots\dots ($$

We need not inquire what is the absorptive power of the thermo-junction, provided that we are justified in assuming that lamp-black surfaces absorb the radiation from the hot tube as freely as that from the sun, or that the constants in these expressions (1) and (2) for the radiation may be taken to be equal.

On balancing, these expressions must be equal, and therefore

$$\frac{pq\theta^4}{(100)^2} \cdot \pi \sin^2 \rho = (T^4 - T_0^4) \frac{\alpha\beta}{d^2}$$

$$= T^4 \left(1 - \left(\frac{T_0}{T}\right)^4\right) \frac{\alpha\beta}{d^2} \dots\dots\dots (3).$$

But $\left(\frac{T_0}{T}\right)^4$ may be neglected, hence we have finally

$$\theta^4 = \frac{\alpha}{\pi d^2} \frac{\beta}{\sin^2 \rho} \frac{100}{p} \frac{100}{q} T^4$$

or

$$\theta = \sqrt[4]{\frac{10000 \alpha}{\pi d^2}} \cdot \sqrt[4]{\frac{\beta}{pq \sin^2 \rho}} \cdot T$$

$$= \frac{0.13806}{\sqrt{\sin \rho}} \cdot \sqrt[4]{\frac{\beta}{pq}} \cdot T.$$

The mean value of $\frac{0.13806}{\sqrt{\sin \rho}}$ is [1.30413].

Therefore

$$\theta = 1.30413 \cdot \sqrt[4]{\frac{\beta}{pq}} \cdot T \dots\dots\dots (4).$$

After a series of observations had been made, the furnace and tube were raised so that the radiation of the latter then passed into the aperture (A), on which the sunlight had previously fallen, while the beam of sunlight was now directed so as to be upon (B), and in this position a second series of observations was taken. The geometrical mean of the result of the two groups gives the Effective Temperature of the Sun, the effect of any difference in the sensitiveness of the thermo-junctions disappearing in the geometrical mean.

Observations were made in the manner described above on August 19th and September 30th, 1901, and reduced by means of equation (4), as exhibited in the following tables. In these the successive columns contain (1) the local mean time, (2) the value of β as read on the micrometer head, (3) the absolute temperature of the tube in the furnace, (4) the sun's altitude, (5) the percentage of the sun's radiation transmitted through the earth's atmosphere, (6) the angle of incidence on the heliostat mirror, (7) the percentage reflected from the surface of the mirror, (8) the corresponding value of θ deduced from equation (4). Of these (5) and (6) and (7) were determined as in *Wilson and Gray's* memoir referred to above.

Note, $\alpha = 7$ mm.

Local time.	Balance.		Altitude.	Per cent. trans.	Angle of incidence.	Per cent. reflected.	☉ Temp. absolute.	Date, August 19, 1901. Blue sky.
	Reading.	Temp. absolute.						
h. m.								First Position (B).
11 0	1.68	1099	47 18	66	62	9	5593	"
11 3	1.69	1098	47 36	66	62	9	5599	"
11 7	1.70	1098	47 44	66	62	9	5598	"
11 19	1.60	1100	48 24	66	62	9	5603	"
11 22	1.55	1100	48 25	66	62	9	5459	"
11 28	1.62	1101	48 41	66	63	11	5161	"
11 30	1.70	1101	48 42	66	64	13	5323	"
11 38	1.75	1101	48 50	66	64	13	5363	"
11 36	1.77	1101	48 54	67	64	13	5378	"
11 38	1.80	1101	49 0	67	64	13	5386	"
				Mean			5394	"
12 48	3.99	1119	48 20	67	68	15	6192	Reversed Position (A).
12 51	4.00	1119	48 13	67	68	15	6184	"
12 53	3.98	1119	48 6	67	68	15	6186	"
12 56	3.96	1119	48 0	67	68	15	6190	"
12 58	3.98	1119	47 40	66	68	15	6210	"
1 3	3.94	1119	47 39	66	68	15	6193	"
1 5	3.93	1119	47 36	66	68	15	6190	"
1 7	3.93	1119	47 24	66	68	15	6190	"
1 9	3.92	1119	47 18	66	68	15	6186	"
1 11	3.96	1119	47 13	66	68	15	6202	"
				Mean			6192	"

Geometrical mean of A and B = $\sqrt{6194 \times 6193} = 5779^{\circ}$.

Note, $\alpha = 5$ mm.

Local time.	Balance.		☉ Altitude.	Per cent. trans.	Angle of incidence.	Per cent. reflective.	☉ Temp. absolute.	Date, Sept. 30, 1901. Weather, slight haze.
	Reading.	Temp. absolute.						
h. m.								First Position (B).
10 45	2.05	1107	31 30	59	64	11	5318	"
10 52	2.16	1112	32 0	59	64	11	5379	"
10 55	2.17	1113	32 0	60	64	11	5364	"
11 25	2.55	1124	33 0	61	63	12	5446	"
11 38	2.42	1124	33 0	61	67	13	5320	"
						Mean.....	5365	
11 51	5.20	1127	33 0	61	68	14	6302	Reversed Position (A).
11 55	6.05	1127	33 0	61	68	14	6585	"
12 0	6.02	1126	33 0	61	68	14	6570	"
12 8	5.39	1127	33 0	61	69	15	6290	"
12 18	5.67	1128	32 30	60	70	17	6201	"
12 25	5.18	1126	32 30	60	70	17	6053	"
12 32	5.25	1127	32 15	60	71	18	5992	"
12 36	5.18	1126	32 0	60	71	18	5968	"
12 50	5.55	1122	32 0	60	71	18	6050	"
12 58	5.25	1122	31 30	59	71	18	5992	"
						Mean.....	6201	

Geometrical mean of A and B = $\sqrt{6365 \times 6201} = 5768^\circ$.

In the above tables Rosetti's determination of the amount of the terrestrial atmospheric absorption has been used. It may be well, however, to give the results obtained by using other estimates of this quantity. Taking Langley's transmission coefficient when the sun is in the zenith as 59 per cent., compared to Rosetti's 71 per cent., the temperature would be multiplied by $\sqrt[3]{(71/59)}$ and thus become 5773×1.054 , which is 6085° absolute. And, as in the previous memoir, to make the case general, if any later investigation shows the zenith transmission coefficient to be x per cent., the effective absolute temperature becomes

$$5773^\circ \times \sqrt[3]{(71/x)}.$$

It may also be of interest to see what effect is produced if absorption in the atmosphere of the sun itself is taken into account. First, considering the falling off in radiation from the central to the peripheral parts of the sun's disc, we may deduce that, if the absorption were everywhere equal to that at the centre, the radiation would be multiplied by $4/3$ and the temperature would become

$$5773^\circ \times \sqrt[3]{(4/3)} = 5773 \times 1.074 = 6201^\circ.$$

Secondly, assuming Wilson and Rambaut's* result for the total loss due to absorption in the solar atmosphere as equal to one-third, our estimate of the temperature would have to be multiplied by $\sqrt[3]{(3/2)}$, and we get finally

$$6201^\circ \times \sqrt[3]{(3/2)} = 6201^\circ \times 1.107 = 6863^\circ \text{ absolute} = 6590^\circ \text{ C.}$$

I wish to express my thanks to Dr. Rambaut for some valuable suggestions during the progress of the work.

"On the Constitution of Copper-Tin Alloys." By C. T. HEYCOCK, F.R.S., and F. H. NEVILLE, F.R.S. Received December 9,—
Read December 12, 1901.

In February, 1901,† we read a short paper on the results of chilling copper-tin alloys, and at the Glasgow meeting of the British Association, 1901, we gave an account of some of the conclusions that we had arrived at concerning the nature of the alloys rich in copper.‡ The present paper extends our conclusions to all alloys of copper and tin, and the accompanying diagram presents the result in a concise but very complete form.

* "The Absorption of Heat in the Solar Atmosphere," 'Proceedings Royal Irish Academy,' 1892, vol. 2, No. 2.

† 'Roy. Soc. Proc.,' vol. 68, p. 171.

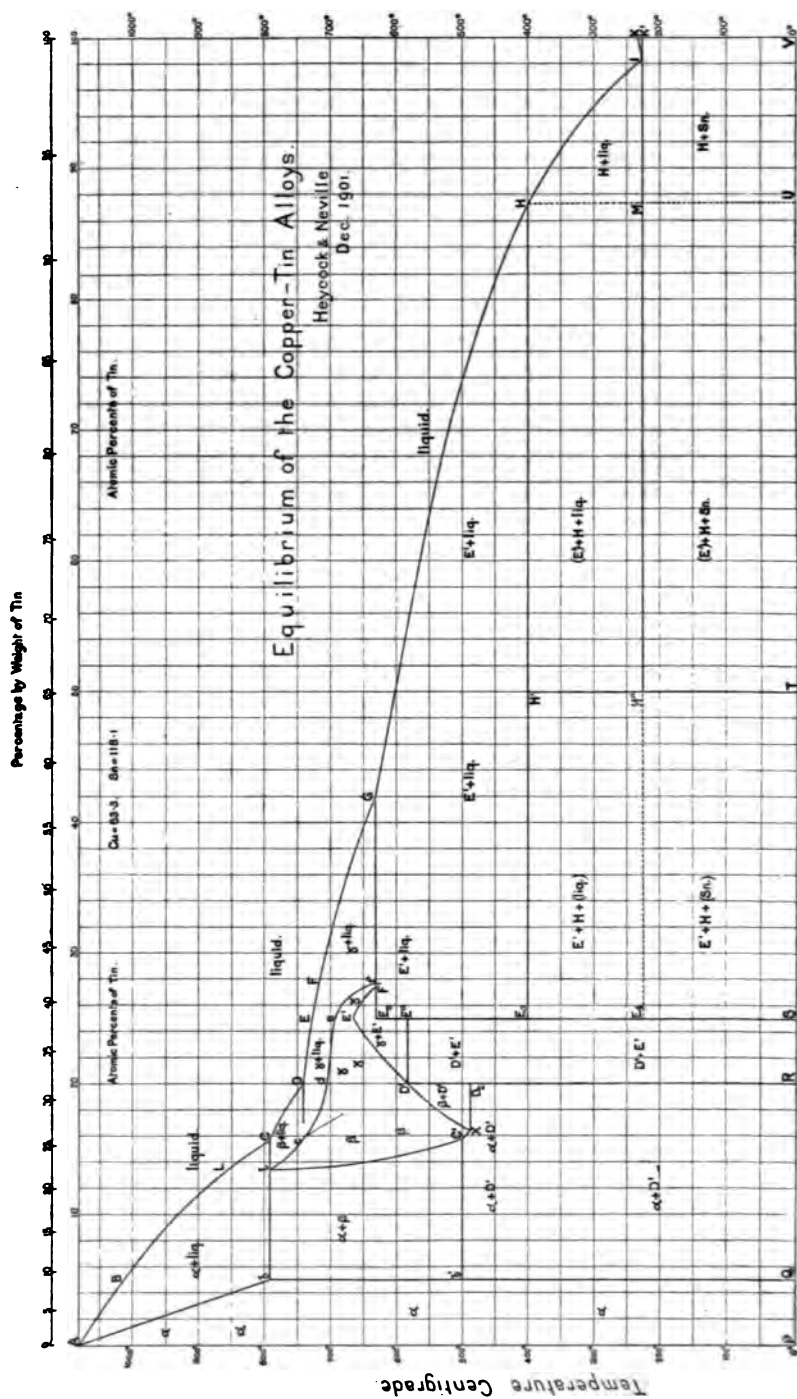
‡ 'Report on Alloys,' Sec. B.

No one dealing with a subject which has been so much studied can give a comprehensive view of the phenomena without largely utilising the results of previous workers. For example, in the pyrometric work needed to determine the freezing-point curve ACDGH of our diagram we have been helped in some important particulars, such as the singularities at G and H, by the results already obtained by Sir W. Roberts-Austen and Dr. Stansfield.* This is still more the case with regard to the evolutions of heat which take place in the solid alloys as they cool, and which are important indications of internal change. The eutectic line $b' C'$, and the very valuable curve XD' E' F', are also due to them. We have, however, re-examined and verified by experiment almost the whole of these pyrometric data.

The microscopic examination of the *unchilled* alloys is also not new, interesting results having been obtained by Charpy and by Stead; for example, the existence of the compounds Cu_3Sn and $CuSn$ has been strongly suspected, if not quite proved. But it is our opinion that conclusive results will never be obtained except by the examination of alloys systematically chilled at selected temperatures, and it is in this method, and in the interpretation of results derived from it, that the new part of the work is to be found. Moreover, even with the help of the chilled alloys, the interpretation would have been very imperfect without the aid of Professor Bakhuis Roozeboom's Theory of Solid Solutions. We are also indebted to that gentleman for many valuable suggestions directly concerning these alloys, and for kind encouragement.

The method and purpose of chilling the alloys can be described in a few words. A number of small ingots of the same alloy, each weighing about 5 grammes, were melted and slowly cooled, and at selected temperatures ingots were withdrawn from the furnace and chilled by immersion in cold water. We thus, to a very large extent, stereotype the structure existing in the alloy at the selected temperature, and we can examine it in the usual way by polishing and etching. If the alloy was partly liquid when withdrawn from the furnace, it generally became more or less granulated by the process of chilling, and the microscope then showed it to consist of large copper-rich crystals formed before the chill and surrounded by a network of much smaller, but very similar, crystals formed during the chill, the whole being embedded in tin-rich mother substance. We can thus determine by the microscope how near to complete solidification the alloy was at the moment of chilling. It was in this way that we traced the branches Ab and $lcdef$ of the solidus. In the above case it is hardly correct to say that the structure existing before the chill is stereotyped by it, but it is sufficiently recorded. On the other hand, when an alloy has solidified before the moment of chilling, further changes are either

* 'Fourth Report on Alloys,' February, 1897.



wholly arrested, or forced to occur on so minute a scale as to be invisible. This is well seen in the photographs 4, 5, 6, of the paper published in February, 1901.*

Inasmuch as the evidence for our statements consists largely of a photographic record of the structure of the various chilled alloys it cannot be given in detail in the present paper, but some of this evidence has already been published in our two papers referred to above. We shall content ourselves at this moment with offering such an explanation of the annexed diagram as will make our conclusions intelligible.

In the diagram the *atomic* percentage of tin in the alloy, and also the percentage by *weight* of tin, are stated at the top. The atomic percentage, being the horizontal ordinate in the diagram, appears as a scale of equal distances, and therefore the percentage by weight is indicated on a gradually increasing scale; but by intrapolation, any intermediate percentage can easily be found. The margin of the figure on the left corresponds to pure copper, that on the right to pure tin. The temperature scale is plotted vertically in degrees Centigrade.

In such a diagram, if we travel down a vertical line from the top to the bottom, we are considering the same alloy (so far as total percentage is concerned) at different temperatures, and, therefore, in different states of aggregation. Each closed compartment of the diagram corresponds to a different state of aggregation, and the phases making up the aggregates are indicated for each compartment. We can, therefore, from the position of the point, determine at once the state of the alloy.

The following phases occur:—

(1) Liquid.

(2) (3) (4) Mixed crystals of three types, α , β , γ .

Each of these is a uniform solid solution containing copper and tin, but of variable percentage as in the case of a liquid solution.

(5) The body E' , which composes the whole alloy at the point E' . This phase is certainly in many cases the pure compound Cu_3Sn , but there may be compartments in which the phase which we shall still call E' is a solid solution of Cu_3Sn and some other body.

(6) The body H , which first appears in certain alloys when they have cooled to a temperature of $400^\circ C$. There are various reasons, some of which we shall give at a later period, for believing that pure H is the compound $CuSn$, but we have not yet obtained conclusive proof that it habitually crystallises in the pure state.

(7) The solid crystallising on the branch IK , which must be pure tin.

Each of the above six solids can exist in contact with the liquid at appropriate temperatures and concentrations.

* 'Roy. Soc. Proc.,' vol. 68.

(8) There is also the substance D' , which is never found in contact with the liquid. The substance D' is pure at the point D' , where we believe it to be the compound Cu_4Sn . This phase will be considered somewhat later.

The relations of the first seven phases can best be stated by considering the solidus (or, as we are inclined to call it, the melting-point curve). The solidus is a broken line consisting of the branches Ab , $ldef$, E_2E_3 , and $H'H''$.

The solidus and the liquidus (or freezing-point curve) are so related that if we draw a horizontal, that is, an isothermal line, cutting the solidus and liquidus, the points of intersection give the percentage compositions of the solid and liquid that can exist in equilibrium at the given temperature. To take an example, the isothermal at $900^\circ C$. cuts the lines Ab and $ABLC$ in points which correspond respectively to a uniform solid containing 3 atomic per cents. of tin, and a liquid containing 11 atomic per cents. These two would be in equilibrium, for when the liquid was cooled it would begin to deposit the solid, and when the solid was heated it would begin to melt and form the liquid.

Whenever a branch of the solidus is sloping, as Ab , or curved, as $ldef$, the solid phase is one of a series of mixed crystals. On the other hand, when a branch of the solidus is vertical, as we have drawn E_2E_3 and $H'H''$, one can infer that mixed crystals are not formed. It is possible that we are wrong in drawing E_2E_3 and $H'H''$ quite vertical; the phase E' may here consist of Cu_4Sn having some H in solid solution, and the phase H may also contain some Cu_4Sn or tin in solid solution, in which cases the solidus would not be a vertical straight line. But we have several reasons, some of which will be stated later, for thinking that the mutual solubility of these bodies is not great.

The angle C of the liquidus indicates that the composition of the solid phase changes abruptly at this temperature, for while the branch ABC corresponds to the solidus Ab , the branch CD corresponds to the solidus lc . The angle C was a great stumbling-block to us so long as we only examined alloys that had not been chilled, but Roozeboom's theory explains in the most perfect manner all the phenomena at this angle. It tells us that just above the temperature C the cooling saturated liquid deposits, and is in equilibrium with, the α mixed crystal whose composition is given by the point b , while just below the temperature C the liquid forms β mixed crystals, much richer in tin and given in composition by the point l . Thus, as the saturated liquid cools through the temperature C an isothermal transformation $\alpha + \text{liquid} = \beta$ takes place. The heat evolved by this reaction is well marked in the cooling curves. No uniform mixed crystals of percentages between b and l can exist. The angle D probably indicates

another break in the series of mixed crystals, but we have not been able to detect a corresponding break in the solidus. However, we propose to call the mixed crystals of the branch *cdef* γ mixed crystals to distinguish them from the β crystals of the branch *lc*, and at Professor Roozeboom's suggestion we have drawn a sloping line from *c* to record the possible gap in the series between the last of the β crystals and the first of the γ .

Thus the branch ABLC of the liquidus deposits α mixed crystals, the branch CD deposits β mixed crystals, and the branch DEFG deposits γ mixed crystals.

The branch GH of the liquidus deposits crystalline plates of the substance E', which is nearly or quite pure Cu_3Sn .

The branch HI deposits crystals of the substance H, which we think is probably CuSn , although it may have an impurity in solid solution.

The liquid of the branch IK deposits crystals of pure tin.

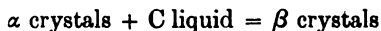
Below the lines *Ablcdef*, E_3H' , and $H''K'$ the alloys should be wholly solid, but on account of imperfect transformations this is not in practice everywhere the case.

The points B, L, C, D, E, F, G, H, I divide the alloys into groups having special qualities. We shall now trace the complete cooling of an alloy from each group.

1. *The AB Alloys, containing less than 5 atomic per cents. of Tin.*—

When these alloys begin to solidify they form, while in the area *AbC*, a mixture of α crystals and liquid. When the temperature of an alloy has fallen below the line *Ab*, it consists of uniform α crystals embedded in a very little tin-rich mother substance, which owes its existence to imperfect adjustment of the equilibrium between solid and liquid during the last stages of solidification. This small residue of mother substance is absorbed at temperatures below *bC* by the α crystals, and modifies their margins, but, in spite of some deceptive appearances, these alloys, at all temperatures below *Ab*, consist substantially of one phase, namely, the uniform solid solution α .

2. *The BL Alloys, containing from 5 to 13 atomic per cents. of Tin.*—These alloys commence to solidify by the formation of α crystals, but when the temperature C is reached the reaction



commences and continues until the C liquid is all exhausted. Thus, when the temperature begins to fall below C, the alloy is wholly solid and consists of α crystals of the percentage *b* embedded in β crystals of the *l* percentage. These β crystals are uniform and play the part of a mother substance to the α crystals, which now grow at the expense of the β , while the solid alloy cools through the range of temperature *bb'*. This growth in the solid is very remarkable in alloys near the

composition l , and accounts for the fantastic angular shapes seen in the α combs of slowly cooled alloys. Our photograph 1, in Plate 2, of the 'Proceedings,' vol. 68, shows this angularity well. In this photograph the α combs are dark. When the temperature falls below 500°C . (the eutectic line of Roberts-Austen and Stansfield) the residual β decomposes into a very minute eutectic of α and the tin-rich body D' , which we believe is Cu_4Sn . Thus at all temperatures below $b'C'$ this group of alloys form a complex of $\alpha + D'$.

3. *The LC Alloys, containing from about 13 to 15.5 atomic per cents. of Tin.*—These alloys commence their solidification with the formation of α crystals, but at the C temperature these are completely changed into β with the percentage l , and then the β reacts along the lines lc and CD with the residual liquid. When the temperature has fallen to the solidus lc the alloy is a uniform solid solution. It is a mass of β crystals, chemically identical, but forming crystalline grains differently oriented and therefore showing, after etching, differences of brightness on tilting or rotating. (The Stead effect.) This uniform solid solution continues to exist until the temperature falls below the line lC' , which is comparable to a freezing-point curve, inasmuch as on cooling to a point on this line, the uniform β becomes saturated with α , and below the line the α crystallises out in large copper-rich crystals. Finally, as before, at 500°C . the residual β breaks up into the C' eutectic. Below the $b'C'$ line these alloys, like the previous group, consist of the complex $\alpha + D'$. We have verified all this repeatedly, chilled samples of the alloy Sn_{14} showing everything very clearly. The C' eutectic has often, of course, been previously observed in unchilled alloys, but, so far as we know, without its real nature being discovered.

4. *The CD Alloys, containing from 15.5 to 20 atomic per cents. of Tin.*—These alloys begin to solidify by forming large, comparatively copper-rich combs of β , and when just solid are a uniform mass of β or γ crystals. This uniform solid solution persists so long as the temperature is above the line $C'XD'$. These solid solutions are very homogeneous, and we have not been able to find any difference between β and γ crystals. When the temperature falls below $C'XD'$, the phase D' crystallises out in ribands at the borders of the crystal grains of solid solution, as well as in the form of a fern leaf or rosette scattered through these grains. The effect is well shown by photograph 6, Plate 3, of the 'Proceedings,' vol. 68, and the cause of the pattern is discussed in that paper. The substance between the crystals of D' is, however, uniform until the temperature X is reached, when the residual solid solution breaks up into the C' eutectic complex of α and D' . The fact that the eutectic point X of the CD alloys is a little lower than C' is evident in the pyrometric curves of Roberts-Austen and Stansfield, and we have abundantly verified it. But Professor Roozeboom suggests,

and we have little doubt on the point, that the true eutectic angle for all alloys from B to D is at C', and that the apparent depression of X is a retardation due to the difficulty experienced by the α in crystallising from the solid solution without a nucleus of its own kind. Thus, in the region XD₂D' the alloys are a complex of β and D', while below XD₂ they form a complex of α and D'. As we have said before, we feel no doubt that this D' is the compound Cu₄Sn.

The alloy Sn₂₀, although it undergoes a well-marked exothermic transformation at the D' temperature, remains substantially uniform. The fact that it has recrystallised is, however, shown by minute traces of the C' eutectic, visible between the large crystals of Cu₄Sn. It may be that the chemical compound Cu₄Sn does not exist above the temperature D'.

5. *The DE Alloys, containing from 20 to 25 atomic per cents. of Tin.*—Between the liquidus and solidus these alloys contain primary combs of γ . On the solidus these combs fill the alloy, and just below it they form a uniform solid solution, but it is very difficult in this region to avoid a commencement of the transformation proper to the D'E' curve. However, our chilled alloys afford abundant evidence that the normal state of alloys between *de* and D'E' is that of a uniform solid solution. When the temperature falls to a point on the curve D'E', long, straight, very uniform tin-rich bars separate out of the solid solution. These are very slender and scanty near D', but become massive and abundant as we approach E', and at that point fill the whole alloy. These bars are really plates of E', seen more or less edgewise, and their appearance of greater or less breadth is partly due to their inclination. These plates, the first appearance of the E' phase, must be either pure Cu₃Sn or mixed crystals of Cu₃Sn and Cu₄Sn; we are not at present able to decide this point. Thus, in the area D'E''E₂F'E', the alloys are a complex of E' + γ . But Roberts-Austen and Stansfield have proved that these DE alloys show, when they fall to the temperature D', an evolution of heat. This must be due to the conversion of the residual γ into D', so that below D'E'' the alloys form the complex D' + E'.

6. *The EF Alloys, containing from 25 to about 27.5 atomic per cents. of Tin.*—These go through the same stages of γ + liq., then pure γ , then γ + E', but at the temperature G the residual γ breaks up into E' and the G liquid.

It may be noted here that the triangular area *lxf* forms an island of typical uniform solid solution, which could only have been discovered by the examination of chilled alloys.

7. *The FG Alloys, containing from 27.5 to 42 atomic per cents. of Tin.*—These alloys, like the preceding, begin by forming the complex γ + liquid; their state, when the temperature G is reached, being γ crystals of the *f* percentage and liquid of the *G* percentage. The isothermal transformation γ crystals = E' + G liquid, now begins.

Cooling curves show that this change is exothermic and abrupt, much heat being evolved by the alloys near f , but little by alloys near G . Herein the transformation at G is the reverse of the transformation at C . The change from the rounded γ crystals to the plates of E' with liberation of liquid is perfectly shown by our photographs of alloys chilled near the G temperature.

When an FG alloy has cooled below G the phase E' crystallises out of a liquid which is becoming richer and richer in tin. This process goes on between the G temperature of 633° and the H temperature of 400° . Below 400° the FG alloys follow the same course as the next group.

8. *The GH Alloys, containing from 42 to about 87.5 atomic per cents. of Tin.*—When these alloys begin to crystallise they deposit plates of E' , and this process goes on until the liquid has the composition H and the temperature is 400° . At this temperature the body E' becomes less stable than the body H , and the reaction $E' + \text{liquid} = H$, commences. This reaction ought to complete itself isothermally until either all the E' or all the liquid is transformed; but in our experiments we find that the reaction is soon arrested through the plates of E' becoming completely coated with H , and thus protected against further action by the liquid. When this has occurred, the temperature begins to fall again, and H crystallises out of the liquid until the eutectic point I is reached.

The result of this imperfect reaction is that we have four compartments in the space E_3SUH , in each of which one of the three phases that exist would be absent if the equilibrium transformations had been completed. This part of the figure assumes that the formula of the H -body is $CuSn$. If this should prove erroneous the line $H'T$ would have to be shifted, but otherwise the diagram would be unchanged. In the four compartments of the area E_3SUH , Professor Roozeboom has suggested the excellent plan of placing a bracket round the symbol of the phase that has no right to be present.

9. *The HI Alloys, containing from 87.5 to 98.3 atomic per cents. of Tin.*—In these alloys the solid first forming is H , and the diagram sufficiently explains itself.

10. *The IK Alloys, with more than 98.3 atomic per cents. of Tin.*—These alloys contain combs of pure tin in a eutectic of $H + Sn$.

Some Observations and Qualifications.

The Solidus.—We have drawn this everywhere as a definite line, but our method of determining it, by the inspection of chilled alloys, is probably not so exact as the method by which the liquidus has been determined, and therefore further work on the solidus is desirable. We think that a determination of the melting point as distinguished from the freezing point would be a promising plan.

The Phase D'.—The great similarity in the character of this phase from the moment when it first appears in the C' eutectic of Sn_6 , to the moment when it begins to appear as fern leaf in Sn_{16} , and gradually grows in amount until it fills the alloy at Sn_{20} , strongly inclines us to think that it is everywhere Cu_4Sn , and not a solid solution isomorphous with this compound. Perhaps the best argument for the view that D' is pure Cu_4Sn can be found in the fact that Roberts-Austen and Stansfield's line of eutectics (XD_2 in our figure) extends to Sn_{20} .

The Phase E'.—Between the percentages D' and E' there is, as we have said, some uncertainty as to the composition of this phase; indeed, at one time we thought that there was another singularity between these points. But on the right of Sn_{25} E' must be pure, or nearly pure, Cu_3Sn . The following experiments bear on this question.

We found that E' and H were but little attacked by hydrochloric acid in the cold, and not at all attacked so long as free tin was present. We therefore chilled the alloys Sn_{35} , Sn_{50} , and Sn_{65} at 450° , a temperature at which they consist only of plates of E' and liquid. They were then exposed for a week or more to strong hydrochloric acid, the acid being tested and changed every day. At first large quantities of tin, but not a trace of copper, dissolved, but gradually less tin came into solution, and when no more tin would dissolve a little copper was found in solution. At this stage the alloy consisted of separate plates of E' and a good deal of black mud. The mud was thoroughly washed away and the plates of E' analysed. Two analyses of each alloy were made.

The Sn_{35} gave 39·1—39·1 per cent. Sn.

The Sn_{50} gave 38·5—38·8 " "

The Sn_{65} gave 41·7—42·2 " "

while

Cu_3Sn requires 38·35 per cent. Sn.

Considering the great difficulty of avoiding the presence of H in the plates of E' analysed, we think the above conclusive.

By similarly treating the alloy Sn_{90} , in which there should theoretically be no E', we obtained a beautiful and clean sample of H in brilliant spear-like leaflets. This contained 61 per cent. of tin, while CuSn requires 65 per cent., so that we have not yet obtained CuSn in a state of purity.

One reason of some weight in support of the formula CuSn is that H must be the body in solution in the liquid alloys of the branch IK of the freezing-point curve; and as we showed in 1890, the value of the depression of the freezing point of the tin points to a molecule in solution containing only one atom of copper.

We have to thank Miss D. Marshall, Lecturer of Girton College, and Mr. W. Fearnside, B.A., of Sidney College, for much help during the experiments.

January 23, 1902.

ir WILLIAM HUGGINS, K.C.B., D.C.L., President, in the Chair.

Mr. Arthur John Evans, Captain Henry B. Jackson, and Professor Charles J. Martin were admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

- I. "Mathematical Contributions to the Theory of Evolution. XI.—On the Influence of Natural Selection on the Variability and Correlation of Organs." By Professor KARL PEARSON, F.R.S.
- II. "On the Correlation of Intellectual Ability with the Size and Shape of the Head. (Preliminary Notice.)" By Professor KARL PEARSON, F.R.S.
- III. "On the Mechanism of the so-called 'Peripheral Reflex Secretion' of the Pancreas. (Preliminary Communication.)" By Dr. W. M. BAYLISS and Dr. E. H. STARLING, F.R.S.
- IV. "A Short Description of the Culicidæ of India, with Descriptions of New Species of Anopheles." By F. V. THEOBALD. Communicated by Professor E. RAY LANKESTER, F.R.S.
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"Mathematical Contributions to the Theory of Evolution. XI.—On the Influence of Natural Selection on the Variability and Correlation of Organs." By KARL PEARSON, F.R.S., University College, London. Received December 20, 1901,—Read January 23, 1902.

(Abstract.)

The influence of directed—natural or artificial—selection on the *characters of a race* is one which it is fundamental for the purposes of *evolution to appreciate quantitatively*. I have already shown in an

earlier memoir of this series the effect of random selection, or what it is better to term random sampling, on the characters of a population. Isolation of a few individuals who form a random sample may produce very sensible modifications of race characters, but it is to directed selection that we must look for changes on the largest scale. The subject is a very broad and complex one—no less than the total effect upon a population containing individuals at all ages of a selective death-rate applied for a long period and a function not only of the organs of each individual, but of the relationship of these organs to each other, and of the stage of growth of the individual. In its complete form the problem presents very considerable difficulties; but if we confine our attention to one class of the population, namely, individuals in the same stage of growth, we are able to trace fairly well the effect upon such a class of selection, however complex may be the relation between the organic characters and the death-rate. Thus we can measure the death-rate which would convert one race into a second by a cataclysmal action on the mean standard deviations and correlations of p out of n possible organs in mature individuals. New complexities arise if the individuals are reproducing themselves during the process of selection, which is then assumed to be continuous and not sudden.

At this point a very definite distinction is reached, namely, that between directly and indirectly selected organs. It may be said that, although it is possible for the recruiting sergeant to select stature, and in so doing differentiate the arm-length of his troop from that of the general population, yet that in natural selection we are given only the modified organs, and so we cannot tell which of them have been directly and which indirectly selected. Both are changed; how discover which was the source of the change? The answer is: In the same manner as we could distinguish between two recruiting sergeants, one of whom selected his troop from the general population by stature, and the other by cubit; in either case the stature and cubit would be both modified, but the mathematical theory of regression would enable us to distinguish between the methods of operating of the two men, and even between them and one who selected by *both* stature and cubit at once. The mathematical theory as developed in this paper shows us that, although the whole complex of characters may have been changed, still, if direct selection have only occurred in p out of n possible cases, there will be certain of the partial regression coefficients which remain unmodified and which will theoretically enable us to distinguish among the whole group of differentiated organs, between those directly selected and those modified only because they happen to be correlated with the directly selected organs. Thus the distinction becomes one of singular importance, for though the selection of a few organs *modifies the means, variabilities and correlations, possibly of the whole*

complex of characters, certain functions of those quantities remain constant, and such constants ought to be discoverable, at any rate in theory, and should serve as the criterion of a common origin, when we deal with local races as having been subjected only to a selection *directly* differentiating a comparatively few characters.

In this memoir the analysis is confined to the case of normal frequency, but most of the chief results are true for all cases of regression. The effects of selection are illustrated in a very considerable variety of cases, especially the influence of selection on the coefficients of heredity is fairly fully dealt with. Tables are given for the simpler cases to enable the biologist at once to appreciate the influence of selection, not only on the size and variability of organs, but on their correlations.

If selection has changed a race from a condition A to a condition B, it becomes of much interest to determine the nature of the selective death-rate by which the process has been carried on, and it is found that this death-rate as represented in the surface of survival-rates enables us to distinguish two kinds of selection, termed in the memoir positive and negative selection. In the first case a race is modified, because the nearer its members are to having their organs with a certain system of values, the better fitted they are to survive ; in the second case the nearer the individuals are to this system the less fitted they are to survive. There will usually be in this second case, not a single system, but an indefinite number of systems which would equally well fit individuals to survive ; in the first case, on the other hand, there are an indefinite number of systems which equally unfit their owners for surviving. This distinction seems of considerable interest.

For example, to select from the French race a race in femur and humerus like the Aino, we should have to proceed by a positive selection ; but to select from the Aino a race like the French, we should have to proceed by a negative selection. To get 1000 Aino we should have to select for these two organs alone out of some 6,000,000 Frenchmen, but to get 1000 Frenchmen from the Aino we must select from about a billion of the latter. Thus we are to some extent able to appreciate the stringency of the selection, which even lasting through long ages, and introducing continuous reproduction, would be needful to enable us to pass in the case of only two organs from one race to the other. Another point brought out by the surface of survival-rates is the fact that the fittest to survive are usually not the most frequent survivors.

It will be seen that the memoir opens up a novel field of investigation, but one so wide that the theory of it must be limited by close *contact with what* is needful for the purposes of evolution. We want *measurements on the local races of animals* to guide us ; at present we

know scarcely in any one case, whether differentiation has taken place by *direct* selection of few or of many organs. When once such measurements are forthcoming we shall have firmer ground to go upon, and the processes of the present memoir seem to suggest how in the future we shall be able to link together quantitatively local races, and possibly at a more remote date obtain quantitative conceptions of the stages of evolutionary descent itself.

“On the Correlation of Intellectual Ability with the Size and Shape of the Head. (Preliminary Notice.)” Drawn up by KARL PEARSON, F.R.S., University College, London. Received January 8,—Read January 23, 1902.

(A New Year's Greeting to Francis Galton, 1902.)

(1.) The collection and reduction of the material on which this preliminary notice is based were due to co-operative labour. Our aim was to ascertain which, if any, physical characters are sensibly correlated with intellectual ability. With this end in view we obtained leave from the Cambridge Anthropometric Committee to freely use their valuable series of measurements on Cambridge undergraduates. Our object was to discover whether these measurements had any relationship to the character of the degrees afterwards obtained by the measured. In order to do this it was necessary to copy the names of the persons measured, and ascertain what was the nature of the degrees ultimately obtained by them. The work of copying the names and colleges of the measured was first undertaken by Miss Mildred E. Barwell, of Girton College, and on her leaving Cambridge was continued and completed by Miss M. Beeton, of the same college. Miss Beeton prepared cards giving the name, college, and chief physical measurements of upwards of a thousand Cambridge undergraduates. This work was very laborious, and considerably increased by the number of duplicates which had to be discarded.* The next stage was to get the subject, place, and character of the degree ultimately taken by the measured placed upon the cards. The labour of tracing each individual in the publications of the University would have been

* There seems to have been a desire on the part of some of the measured to test the accuracy of the measurer by repeating the process as often as possible, and subjecting him to various artifices. One senior wrangler was measured no less than five times! Considering that the measurer had not the means of a prison warder for controlling his subject, he appears to have managed fairly well. When the duplicates were hopelessly irreconcilable—generally in those characters depending upon the agency of the subject—they were all rejected. In other cases where the differences were slight, the first measurements were taken as representative, and the later cards thrown out.

excessive, and we owe entirely to the kindness of Mr. W. H. Macaulay, of King's College, the presentation of our problem to the authorities of the University Registry and the arrangements for supplying the necessary data. We have heartily to thank both him and the officers at the Registry for aid in this matter. We thus obtained the addition to our cards of the exact nature, honours or poll, class-place, subject (science, theology, literature, &c.) of the degree taken by each individual. We were then provided with a most valuable mass of material for testing how far any of the chief physical characters are correlated with a fairly comprehensive scale of ability, or with the special intellectual tastes of the measured.

There is work in this mass of material, reducing and classifying it, for one or two good calculators during several years. At present no attempt has been made to reduce it, except in one special direction—that of the correlation of intellectual ability with the shape of the head. This is the subject of the present preliminary notice. The tables in this case were prepared partly by myself and partly by my assistant, Mr. E. Blanchard, B.A., of Caius College. Nearly the whole work of calculation is due to Dr. Alice Lee and Miss M. A. Lewenz, B.A. The conclusions, therefore, are a co-operative product of the biometric workers associated with me at University College, London.

(2.) Dr. Lee, in a paper "A First Study of the Correlation of the Human Skull," published in the 'Phil. Trans.,' A, vol. 196, pp. 225-264, has presented a considerable amount of evidence to show that "there is no marked correlation between skull capacity and intellectual power" (p. 259). We have found this result frequently contested and a very definite statement made that able men have large heads. We cannot find, however, that there are really reliable statistics, adequately treated, which in any way prove this general statement. It is perfectly true that the professional classes in this country have a rather larger head than the hand-working classes, and the former are rather more intellectual; but they are taller and physically more developed also, and the whole difference is most probably due to better nurture. One of our number, Dr. W. R. Macdonell, has recently shown that the head of the Cambridge undergraduate is larger than the head of the criminal population,* but any deduction from a mixture of these two classes (that ability is correlated with size of head) would be wholly misleading. We must take a homogeneous class of approximately the same nurture and habits, and inquire whether there is any correlation between ability and size of head within this class. It is this which we have attempted to do with the Cambridge statistics.

(3.) For our present purposes we have made a very broad classification of Cambridge men into poll and honours men. There are occasionally poll men who undoubtedly are intellectually stronger than some

* "On Criminal Anthropometry . . .," 'Biometrika,' vol. 1, pp. 185, 188, &c.

honours men. But to any one acquainted with the present Cambridge system the division thus made will appear a well-marked and distinctive one. I think the majority of college tutors would undoubtedly agree in the broad statement, that the poll man cannot be compared for intellectual ability with the honours man. Making this broad division, we are at once able to use the method of my paper: "On the Correlation of Characters not Quantitatively Measurable."*

The characters dealt with from the Cambridge measurements were (a) Cephalic Index; (b) Length of Head; (c) Breadth of Head. The classifications adopted were: for (a) indices under and over eighty; for (b) lengths under and over 7·65 inches; and for (c) breadth under and over 6·05 inches.

The following fourfold tables resulted from the classification:

(a.) Cephalic Index.

		Under 80.	Over 80.	Totals.
Ability.	Honours	307·5	216·5	524
	Pass	276·5	210·5	487
	Totals	584	427	1011

The correlation therefore between ability and dolichocephaly is $r = 0·0305 \pm 0·0349$.

(b.) Length of Head.

		Under 7"·65.	Over 7"·65.	Totals.
Ability.	Honours	264	260	524
	Pass	272	215	487
	Totals	536	475	1011

The correlation† therefore between short heads and ability is $r = -0·0861 \pm 0·0332$.

* 'Phil. Trans.,' A, vol. 195, pp. 1-47.

† This result is due jointly to Miss M. Lewenz, B.A., and Dr. Alice Lee.

(c.) Breadth of Head.

		Over 6''·05.	Under 6''·05	Totals.
Ability.	Honours	284	240	524
	Pass	250	237	487
	Totals	534	477	1011

The correlation therefore between ability and broad heads is $r = 0.0450 \pm 0.0322$. Were these results sensible we should have to conclude that ability was directly correlated with increased length and breadth, and further with a dolichocephalic tendency. But if we compare the numbers with their probable errors, we see that the correlation is non-significant in the cases of both cephalic index and breadth; while in the case of length of head, the correlation is only two to three times the probable error, and, even if this be taken as significant, which is doubtful, the value of the correlation is so small that it can have no practical importance, when we attempt to judge ability from physical measurements of the head.*

So far then as our Cambridge results go, they thoroughly confirm Dr. Lee's investigation as to the capacity of the skull. There is no marked correlation between ability and the shape or size of the head.

(4.) It seemed possible—although on less safe data—to consider the same problems from our school-measurement series.† I say on less safe data for two reasons: (a) Because the measurements being made in schools of all types up and down the country really involve a mixture of classes with very different forms of nurture, and (b) Because we have to allow rather roughly for growth.

There is little doubt that the cephalic index remains sensibly constant during growth, and therefore in this character we can compare children of different ages.‡ On the other hand, the length and breadth

* [It was suggested to me when this paper was read that a better grouping would be first and second class honours men together and third class honours men with poll men. Accordingly, Miss Lewenz undertook the calculations for this division. She found the correlation between ability and short heads -0.1066 ± 0.0343 , and between ability and broad heads 0.0347 ± 0.0345 . The latter correlation was thus practically unaltered, and the former increased only within the limits of the probable error. Compare the results in the summary for honours men only.]

† This series was carried out by assistance from the Government Grant Committee.

‡ I find for Cambridge men, cephalic index = 79.5; for school children (*all classes*) 78.4, and Dr. Macdonell for criminal classes 78.5. Compare also the valuable

of the head changes, and we are compelled to reduce our measurements to what they would be if the children were all of one age. Taking the length of head, Dr. Alice Lee formed a correlation table between length of head and age for 1856 boys, ranging from 4 to 19 years of age. The mean head-length was taken for each year of age, and thus a curve of average size of head of boys from 4 to 19 obtained. This curve presented several marked features, notably an apparent period of rest in growth during the 12th year.* These features will be more fully discussed on another occasion, but the rest in the 12th year is referred to here as a reason why we have chosen the 12th year as a standard age to which we may refer all our measurements. The growth of the average boy from every year up to the 12th year, and from the 12th up to every later year, was now determined. These growths were then added to or subtracted from the head-length measured on the individual in order to deduce from the observed size of the boy's head his approximate size of head in the 12th year. This, of course, is far from legitimate; it supposes that each individual boy grows like the average boy, which we know is not the fact. Still it will not lead us very far astray for the purpose of the broad classification we are proposing.†

A precisely similar process was adopted for the breadth of head. The 12th year was still chosen for the standard age, although in this case the growth-curve from 4 to 19 years exhibits no period of rest, being almost exactly a straight line.‡

In our school observations the following classification of intelligence was adopted (it having been shown by experiment to give fairly concordant results when the same children were classed by independent observers):—*Quick-Intelligent*, *Intelligent*, *Slow-Intelligent*, *Slow*, *Slow-Dull*, and *Very Dull*. For the purposes of our present investigation, we grouped the first two categories together and the remaining four together. We will call the first group *Intelligent* and the second *Slow*, and these may be taken to roughly correspond to our first division of *Honours* and *Poll* men when dealing with Cambridge graduates.

results on this point obtained by Dr. W. Pitzner, 'Zeitschrift für Morphologie u. Anthropologie,' vol. 1, 1899, p. 372.

* A like but less marked rest in the 12th year is indicated in W. T. Porter's curves for growth of head in boys: see "The Growth of St. Louis Children," 'Trans. Acad. of Science of St. Louis,' vol. 6, pp. 264-380, Plate 32.

† An examination of Mr. Porter's curves of growth for the 25th, 50th, and 75th percentile grades (*loc. cit.*, Plate 32) seem to indicate (by the parallelism of these curves) how small really is the correlation between amount of growth and actual length of head.

‡ This is again in keeping with Mr. Porter's results for ages 6 to 17; see *loc. cit.*, Plate 33. His percentile curves are again approximately parallel, and thus favour the view that amounts of growth are largely independent of size. Why there is rest in growth of length but not in growth of breadth is an interesting problem.

The following tables resulted from this classification :—

(a.) Cephalic Index.

Ability.		Under 78·5.	Over 78·5.	Totals.
	Intelligent	426·75	475·75	902·5
	Slow	519·25	571·25	1090·5
	Totals	946	1047	1993

The correlation deduced from this table between ability as measured by the teacher and dolichocephaly is $r = 0\cdot0052 \pm 0\cdot0240$.

(b.) Length of Head. (Reduced to 12th year.)

Ability.		Below 184·5 mm.	Above 184·5 mm.	Totals.
	Intelligent	404·5	473	877·5
	Slow	536	560·5	1096·5
	Totals	940·5	1033·5	1974

The correlation, therefore, between short heads and ability is $r = -0\cdot0437 \pm 0\cdot0242$.

(c.) Breadth of Head. (Reduced to 12th year.)

Ability.		Below 145 mm.	Above 145 mm.	Totals.
	Intelligent	399·5	499	898·5
	Slow	542	545·5	1087·5
	Totals	941·5	1044·5	1986

The correlation, therefore, between ability and broad heads is $r = 0\cdot0843 \pm 0\cdot0240$.

In judging these tables we make the important assumption that the teacher's estimate of the ability of a boy at a given age is approximately correct if applied to him when 12 years old. There will of course be exceptions to this rule, but they will hardly be numerous enough to invalidate the results drawn from such broad classifications as we are here dealing with.*

These results confirm entirely the conclusions we have drawn from the Cambridge statistics. There is a non-significant correlation between dolichocephaly and ability; there are very small correlations between length and breadth of head and ability. The ability and length correlation here is about what the ability and breadth correlation was in the Cambridge case, and *vice versa*. Hence we cannot assert that either length or breadth is dominant in the case of ability.

Summary.—If we sum up the conclusions which can be drawn from our present material, I think they would run as follows:—

We have taken two standards of ability: (i) a youth's view of his own capacity (doubtless influenced by the opinions of his parents and teachers), determined by whether he works for a pass or honours degree; (ii) the teacher's view of the child's capacity. In neither case is there a sensible relation between ability and shape of the head as judged by the cephalic index.

In both cases there is a small correlation between the size of the head as judged by both length and breadth and the individual's ability. The mean of the values found gives $r = 0.0649$ for length and ability and 0.0647 for breadth and ability, or taking these as the same, we may say that the correlation between size of head and ability is 0.0648 , practically 0.065 .

Let us examine this numerically to realise better its degree of significance. Consider the class of people who have an ability which occurs only in 2 per cent. of the population—a fairly high standard. Let h be their grade of intelligence and σ the standard deviation of intelligence; so that 2 per cent. of the population have an intelligence differing from the mean by h or more. Then to find h/σ we have:

$$\frac{1}{50} = \frac{1}{\sqrt{2\pi}} \int_{h/\sigma}^{\infty} e^{-\frac{1}{2}x^2} dx,$$

whence, by tables of the probability integral:

$$h/\sigma = 2.05375.$$

Let y be the mean size of head of these exceptionally able people and σ' the standard deviation of size of head; $r = 0.065$, and N = total population. Then:

* As a teacher, I am continually struck by the accordance between one's *general* appreciation of a student's power—not necessarily on an examination-room scale—and his after-achievement in life.

$$\begin{aligned}\bar{y} &= \frac{\frac{N}{\sqrt{2\pi}\sigma} \int_h^\infty e^{-\frac{x^2}{2\sigma^2}} \frac{r\sigma'}{\sigma} x dx}{\frac{N}{\sqrt{2\pi}\sigma} \int_h^\infty e^{-\frac{x^2}{2\sigma^2}} dx} \\ &= \sigma' \frac{r \frac{1}{\sqrt{2\pi}} e^{-\frac{h^2}{2\sigma^2}}}{\frac{1}{\sqrt{2\pi}}} = \sigma' \times 0.156,975.\end{aligned}$$

Now let us find the proportion of the population which has a head as large as the mean head of the exceptionally able people. We have if n be their number :

$$n/N = \frac{1}{\sqrt{2\pi}} \int_{0.156,975}^\infty e^{-\frac{1}{2}y'^2} dy'.$$

We easily find from the tables $n/N = 0.438$. Thus 44 per cent. of the population have heads as large or larger than the mean head of the exceptionally able 2 per cent. Conversely, 44 per cent. of the population are as able or abler than the 2 per cent. of the population with exceptionally big heads. When we recollect that 50 per cent. of the population would be abler or larger headed than the mean of the population, it will be recognised how small is the basis upon which we can argue from ability to largeness of head or from largeness of head to ability.

In dealing with this problem we have dealt with a normal distribution of ability in the general population. There is nothing thus far to prove a *close* relationship between ability and either shape or size of the head. If phrenology were not a discredited branch of knowledge it would be easy to investigate its claims by a like statistical method. But phrenology has not met with the same widespread acceptance among men of science as the belief that ability—brain power—is correlated with the size of the head has done. Of course if abnormal ability—genius—and abnormal dulness—crass stupidity—were *both* associated with large-headedness, our method of investigation would fail to exhibit this result. We have no means of isolating crass stupidity from the mass of poll men. But we are able to pick out from the honours men—unfortunately only 524 in number—the grades of ability corresponding to first, second, and third classes under competitive examination. I found the following distributions :

(a.) Cephalic Index of Honours Men.

Honours.	Under 80.	Over 80.	Totals.
1st class	88	65	153
2nd class	118·5	68·5	182
3rd class	106	83	189
Totals	307·5	216·5	524

Miss M. A. Lewenz worked out the correlation, first grouping the first and second class together, and then the second and third class to form a fourfold table. In the first case we have correlation between ability and dolichocephaly = $0\cdot0641 \pm 0\cdot0487$, and in the second case = $-0\cdot0254 \pm 0\cdot0490$. The probable error is of the same order as the coefficients, and neither result is significant. We conclude that there is nothing whatever to show that great ability as evidenced by the competitive-examination test is related especially to either dolichocephaly or brachicephaly.

(b.) Length of Head of Honours Men.

Honours.	Under 7"·65.	Over 7"·65.	Totals.
1st class	68	85	153
2nd class	94	88	182
3rd class	102	87	189
Totals	264	260	524

Grouping first and second class together, Miss M. A. Lewenz found $r = 0\cdot0865 \pm 0\cdot0471$ for the correlation between great ability and long-headedness, sensibly the same as that found on p. 335, when the poll men were taken into account. Grouping second and third class together, the value of r rose to $0\cdot1263 \pm 0\cdot0439$, the probable error of the difference being greater than the difference. Before commenting on this I give the table for breadth of head :

(c.) Breadth of Head of Honours Men.

Honours.	Under 6" '05.	Over 6" '05.	Totals.
1st class.....	58	95	153
2nd class	95	87	182
3rd class	87	102	189
Totals	240	284	524

Grouping first and second class together Miss M. A. Lewenz found $r = 0.0056 \pm 0.0475$ for the correlation between great ability and breadth of head; while when the second and third classes were grouped together r rose to 0.1689 ± 0.0478 , the probable error of the difference being thus sensibly less than the difference. Now if such a small number as 524 can be trusted there would thus seem to be an increasing correlation between ability and size of head, when we isolate the exceptional men who take first-class honours.* But when we note the size of the probable error of these results, and see how comparatively small they are, it seems possible to attribute their divergence from the previous values to other sources than the relation of genius to size of head. Notably we must bear in mind that head length and breadth increase throughout life;† and that the honours men as a whole are older than the poll men and older in the first than in the second class—a considerable number of resident dons being included in this series of measurements—men of high honours and considerable age. Hence I think we can lay only slight stress on the apparent increase of correlation as we isolate the more brilliant men. We must finally conclude that: Very brilliant men may possibly have a very slightly larger head than their fellows, but taking the general population there is really a very insignificant association between size of head and ability. For practical purposes it seems impossible, either in the case of exceptionally able men or in the bulk of the population, to pass any judgment from size of head to ability or *vice versa*.

* The alteration of correlation with the alteration of the class divisions shows that the frequency surface is not accurately normal. In the *general* Cambridge undergraduate population, as well as in the criminal classes, the head measurements give normal curves within the limits of random sampling. See Macdonell, *loc. cit.*, p. 181, *et seq.*

† W. Pflüger, 'Zeitschrift für Morphologie u. Anthropologie,' vol. 1, p. 365.

"The Affinity of Tmesipteris with the Sphenophyllales." By A. P. W. THOMAS, M.A., F.L.S., University College, Auckland, New Zealand. Communicated by Professor G. B. HOWES, F.R.S. Received January 6,—Read January 23, 1902.

The peculiar fertile structures of Tmesipteris and Psilotum have given rise to much discussion as to their morphological nature. The foliage leaves in Tmesipteris, as is well known, are simple, whereas the fertile structures are forked, that is, each has a stalk bearing leaf-like lobes, a bilocular sporangium or synangium being attached to the stalk just below the lobes.

Three views have been taken as to the nature of these fertile structures—

1. The whole structure has been interpreted as a branch of the stem, being equivalent to a reduced Lycopodium spiko. The lobes, according to this view, will be leaves. (Juraniyi, Strasburger, Sachs, Goebel.)
2. It has been considered as equivalent to a single leaf. Brongniart and Luerssen compared it with a Lycopodium sporophyll. Professor Bower, as the result of his investigation of the development of the organs of Tmesipteris, regarded it as a "single leaf with two lobes, bearing the synangium on its adaxial face."* He considered the synangium to be a septate sporangium.
3. Dr. Scott considers that an at least equally probable explanation is, that the synangium with its axis may correspond to the ventral sporangiophore of the Sphenophyllales.†

I propose to adduce evidence to show that whilst Professor Bower's view that the whole fertile structure of Tmesipteris is equivalent to a single leaf is correct, Dr. Scott's suggestion that the synangium, with its axis, corresponds to a ventral sporangiophore, is also correct.

Professor Bower speaks of the whole fertile structure as a sporangiophore, whereas Dr. Scott would restrict the term sporangiophore to the synangium and its axis, referring to the Sphenophyllales, in which sporangiophores spring from the upper surface of bracts as long pedicels, bearing one or more sporangia at their ends. To avoid confusion, I propose to speak of the whole fertile structure as a fertile leaf or sporophyll, and will use the term sporangiophore in the sense of Dr. Scott and Mr. Seward.

The evidence I have to offer is derived from the study of the variations which occur in the sporophylls. These variations are of striking

* 'Phil. Trans.,' B, 1894.

† 'Studies in Fossil Botany,' p. 499.

character and abundant, and appear to place the matter in a very clear light, unless, indeed, we refuse to accept evidence drawn from any variations. I should state here that Professor Bower has already used certain variations (chiefly variations in the way of reduction) of the sporophylls of *Tmesipteris* as evidence as to the nature of the synangium. But with one partial exception he, apparently, did not observe the variations which I propose to describe here. But the material available for his investigations is stated to have been of limited amount, and it is clearly in the native country of *Tmesipteris*, that the question can be investigated to the greatest advantage, for there the quantity of material available is practically unlimited, and the conditions under which the plants are growing can be best studied.

The variations now to be described may be roughly arranged in three categories—

1. Sporophylls with repeated dichotomy and 2 or 3 synangia.
2. Sporophylls in which the synangium is not sessile but raised up on a stalk or pedicel.
3. Sporophylls in which the synangium is replaced by a leaf lobe of normal appearance.

1. A common variation of the sporophyll is that one of the branches forks a second time, a second synangium of normal form being carried just below the second fork. That is, a single sporophyll carries two synangia, and there are three flattened leaf lobes. A less common variation is for both of the branches of the first dichotomy to fork again. The stalk of the leaf divides dichotomously into two branches, which are rather stalk-like in character; each of these, after reaching a certain length, forks again, the branches this time becoming flattened and leaf-like in form; the whole leaf thus terminates in four leaf-lobes or leaflets, of the same character as the two leaf-lobes of the commoner type of sporophyll. There is a synangium not only at the first fork, but also at each of the forks of the second order. The three synangia are all of normal form and character, and may be of practically equal size.

It will be remembered that sporophylls and vegetative leaves commonly alternate in zones on the same shoot. Professor Bower has stated that, on the whole, abnormalities in the sporophylls and synangia commonly occur at the beginning or end of a fertile zone, as if the abnormal condition might be referred to less perfect conditions of nutrition. Whilst I have found this to be true for variations of reduction in the leaf-lobes or synangia, it does not apply to these cases in which the sporophyll shows a development in excess of the normal. On the contrary, they occur rather towards the middle of a fertile zone, often many together, and with neighbouring normal

sporophylls perfectly developed. It would seem, then, as if the greater development of the sporophylls occurred when the nutritive conditions were most perfect. May we infer that, if the nutritive conditions remained equally satisfactory throughout, the whole of the sporophylls would show repeated dichotomy?

Professor Bower has observed that those abnormalities which occur most frequently in a given species will be those which are most worthy of consideration for morphological argument. If we accept this statement as a reasonable one, the variations now under discussion appear to deserve serious attention. For they occur with considerable frequency, as I will show; and though we may, in a sense, apply the term abnormality to them, it is in a sense with which nothing of the nature of a pathological variation can be associated. They are of healthy appearance, and occur in the most vigorous parts of the best shoots. I have found as many as five or six sporophylls with repeated dichotomy in a single fertile zone, whilst shoots with three such sporophylls are fairly numerous. In the course of half-an-hour's search in the forest I can always count on finding several shoots with such variations. But this applies only to those districts where favourable conditions for the growth of *Tmesipteris* are best realised; in less favourable localities the variations may certainly occur, but not in such numbers. It is probably not going too far when we assert that, when conditions are most favourable, the sporophylls of *Tmesipteris* normally show a repeated dichotomy.

2. In a second group of variations we find that the synangium, instead of being sessile on the petiole of the leaf just below the fork, that is, the point from which the leaf-lobes diverge, is raised up on a longer or shorter stalk. I have found a considerable number of these variations, and they show a good deal of diversity. Sometimes the synangium is carried up to a height equal to half the length of the synangium, and lies transversely to the axis of the whole leaf. The two lobes of the synangium then appear to be balanced on the summit of the pedicel, hanging down somewhat, one on each side, so as to suggest a peltate sporangiophore. In other cases the synangium retains its direction between the leaf-lobes, but revolves on a transverse axis, so that the longitudinal groove of the synangium, by which dehiscence takes place, faces outwards between the leaf-lobes, instead of looking rather towards the axis of the shoot. One would think that the outward position would, on the whole, be more favourable for the dispersal of the spores.

3. In the third group of variations no synangium appears, though the leaf has otherwise the character of a sporophyll, and is forked. Professor Bower has described, and I, too, have seen, many such sporophylls in which a minute scar appears in a position below the fork,

doubtless representing an abortive synangium. Professor Bower has also figured* a sporophyll, in which a long process arises in the place of the synangium. The process in the figure, though long in proportion to its width, is still only half the length of the leaf-lobes. This was interpreted as a correlative vegetative growth. A greater amount of material has shown what the correlative growth may become. I have found a large number of variations in which the growth reaches a greater development, until we get sporophylls in which the place of the synangium is occupied by a leaf-lobe of normal appearance. There can be no mistaking the appearance of this leaf-lobe—the form and structure of the normal leaf-lobe or leaflet of a sporophyll are exactly reproduced even to the details of the peculiar mucronate termination. What is the natural conclusion from the appearances described? Is it not that the synangium of *Tmesipteris* is morphologically equivalent to a ventral leaf-lobe? Or some writers would possibly prefer to say that the structure which carries the sporangium (or sporangia) is a ventral leaf-lobe, the sporangia being held to be organs *sui generis*, which may be carried in various positions, in the present case on a leaf-lobe. If so, we must suppose that the leaf-lobe in the normal synangium is reduced to the base, and probably the axis of the synangium.

It should be added that often two or three of the sporophylls at the distal end of a fertile zone may show this vegetative development of the ventral leaf-lobe, suggesting that after the primordium of the sporophyll has been developed to a certain stage, the particular nutritive conditions which lead to reproductive development have failed, and that there has accordingly been a relapse to the vegetative condition. Hence the primordium of the synangium has developed into a leaf-lobe of the ordinary structure.

There is little difficulty in stating the character of the variations to which *Tmesipteris* is subject, but to attempt to apply these for the purpose of tracing the affinities of the plant is a more hazardous matter, and there is doubtless room for differences of opinion.

Tmesipteris and its ally *Psilotum* have usually been classed with the *Lycopodiinae*, though it has been admitted that they are rather outlying members of the class. It has been the difficulty in reconciling the sporangium-bearing structures of the *Psiloteae* with the usual simple sporangia and sporophylls of the typical *Lycopodiinae*, which has caused so much morphological discussion as to their nature.

We are led, therefore, to inquire in what other group of *Pteridophytes* do we find dichotomous division of leaves, sporophylls more elaborate than vegetative leaves, several synangia or groups of sporangia on a single sporophyll, and the presence of a special ventral lobe of the leaf as sporangiophore.

* 'Phil. Trans.' B, 1894, Plate 52, fig. 152.

Leaving on one side obvious comparisons as regards some of these characters with certain ferns, I would venture to suggest that it is in the extinct Sphenophyllales that we find the closest morphological parallel with the sum total of the characters now shown for Tmesipteris. The leaves in Sphenophyllum were frequently heteromorphic. A common type of leaf was a wedge-shaped one, but it is important to notice that the veins were dichotomously divided, and the margin of the leaf frequently more or less notched or lobed in accordance with the venation. But the leaves varied much in form, from such as have been described to leaves with a lamina dissected into dichotomously branched linear segments, or to simple narrow uninerved leaves.* In *S. cuneifolium* the upper cone-bearing branches show the finely cut foliage, whilst in some specimens entire and much divided leaves occur mixed together in the same specimen.

But it may be objected that such leaves are foliage leaves, whereas it is the sporophylls which alone are forked in Tmesipteris. The sporophylls of Sphenophyllum formed strobili, as a rule sharply marked off from the foliage-bearing regions, but in *S. trichomatosum* the cones were very lax, and not sharply marked off.† In the well-known form *S. Dawsoni*, the bracts were simple, but in others they were forked. In *S. tenerrium* the cones were small, but the bracts narrow and dissected.

The force of the comparison with the Sphenophyllales is of course intensified if it be admitted that the variations recorded above for Tmesipteris prove that the synangium is equivalent to a sporangiphore with its sporangia.

In *Sphenophyllum Dawsoni* each bract carried two sporangiophores on its upper surface, each sporangiophore bearing a single pendulous sporangium. But in the *Bowmanites Roemeri* each sporangiophore bears two sporangia. If we may trust the variations recorded under Group 2 above, we have here a surprisingly close correspondence between Bowmanites and Tmesipteris. But some species of Sphenophyllum present evidence of as many as four sporangia to a single sporangiophore. I have occasionally found trilocular synangia in Tmesipteris, as, indeed, others have done; whilst in Psilotum the number of loculi, though normally three, may vary from two to five.

Perhaps one of the most interesting forms for comparison with Tmesipteris is the cone described by Scott under the name of Cheirostrobos, and shown by him to belong to the Sphenophyllales.‡ In Cheirostrobos the sporophylls are very elaborate; each is divided nearly to its base into three lower sterile segments and three upper fertile segments or sporangiophores. May we compare a sporophyll of

* Seward, 'Fossil Plants,' vol. 1, p. 391.

† Scott, 'Studies in Fossil Botany,' p. 105.

‡ Scott, 'Phil. Trans.,' B, 1897.

Tmesipteris with three synangia or sporangiophores, with the sporophyll of *Cheirostrobis*? There is, indeed, considerable difference in details; thus the division of the sterile segment in *Cheirostrobis* is palmatifid, whereas the division of the Tmesipteris sporophyll is dichotomous. But this difference is probably of little importance, for in any case we cannot expect to show more than a general correspondence. It is much that we can show in each case an elaborate sporophyll, with three ventral sporangiophores.

Cheirostrobis, although a synthetic form, had probably the most complex cone known amongst the Pteridophytes, and this was doubtless specialised in some particulars. It may be added that the number of sterile segments and sporangiophores in *Cheirostrobis* might be reduced to two, and we may compare this with those variations of the sporophylls of Tmesipteris in which only two sporangiophores occur. Further, the vascular bundle of each sterile segment bifurcates at the base of the lamina, and the branches run out into two up-turned processes, that is, the last division of the bundles is dichotomous.

If we admit the homologies suggested here, we may draw an interesting parallel between Tmesipteris and the Sphenophyllales on the one hand, and such a simple Lycopodium as *L. selago* and *Lepidodendron* on the other hand. Tmesipteris and *L. selago* are both relatively small forms, which show an alternation of zones of sterile and fertile leaves, whereas the Palæozoic plants both showed more complex vegetative structure, with secondary increase and specialised cones.

There are certain other points, however, to be considered before the affinity between Tmesipteris and the extinct Sphenophyllales can be considered as established. As regards the anatomical structure, the difficulty does not appear to be great. I do not propose to enter fully into this question at present; it will suffice to quote Scott and Seward on this point. The former states that the Psilotæ "are anatomically perhaps the nearest to the Sphenophylleæ," whilst Seward considers that "the anatomical characters of the Sphenophyllum shown are such as one finds in some recent genera of the Lycopodiinæ, especially *Psilotum*." But *Psilotum* is unquestionably closely allied to Tmesipteris.

The character in which a greater contrast exists is the arrangement of the leaves. In the Sphenophyllales the arrangement of the leaves, both vegetative and fertile, in whorls, is a striking feature, whereas in the Psilotæ the leaves are scattered. This difference, however, can hardly outweigh the evidence afforded by the other characters.

The Sphenophyllales have been recognised as intermediate in their characters between the Equisetales and the Lycopodiales; even on the assumption, therefore, that the Psilotæ are nearer the Sphenophyllales, it would not be surprising to find they possess some Lycopodiinæ characters.

Dr. Scott has expressed the opinion* that the Psilotæ are clearly very remote from the Lycopodiæ, and has suggested the hypothesis that they may have branched off from the main line of Lycopod descent very far back, at a point where some of the characters common to the Sphenophyllales were still retained.

If the evidence I have endeavoured to adduce here as to the character of the sporophylls, and especially the nature of the synangium, as a ventral leaf-lobe with its sporangia, be admitted, it will follow that the relationship of Tmesipteris and Psilotum with the Sphenophyllales is much closer than has generally been allowed, and possibly even closer than was supposed by Dr. Scott. So; too, it will follow that the Psilotæ are more remote from the typical Lycopodinæ than has been supposed.

Addendum—Received January 21, 1902.

It will obviously be of importance to examine the sporophylls of Psilotum, the only close ally of Tmesipteris, in order to see whether any facts can be established which bear upon the inferences drawn from the study of Tmesipteris. The supply of material available when the above note was written was hardly sufficient to enable me to form a definite conclusion, but I have since obtained an abundant supply of *Psilotum triquetrum*. Before describing the results obtained, it will be well to refer to a memoir by Solms-Laubach,† in which certain observations of the sporophylls of Psilotum are contained. Unfortunately, the original memoir has not yet been accessible to me, so that I can only judge of its contents from references by Bower‡ and elsewhere. But Bower saw nothing in the variations described inconsistent with the hypothesis that the sporophyll is a single leaf with two lobes, and the synangium merely a septate sporangium. But the character of the sporophylls and their variations in Psilotum is by no means so obvious as in Tmesipteris, for the leaves are greatly reduced, *Psilotum triquetrum* being largely a xerophyte, and though the decurrent bases of the leaves are distinct, the free portions are very small, and often all but free from chlorophyll. Nevertheless, when we compare the sporophylls of Psilotum with those of Tmesipteris, it becomes clear that they are essentially similar. We find fairly numerous instances in Psilotum of a second dichotomy of one branch of the first fork, or, less frequently, of both branches. In the former case we find two synangia and three leaf-lobes, here very minute; in the latter case we find three synangia and four leaf-lobes. When there are two synangia we find the attachment of the one is nearer the base of the sporophyll than the other.

* 'Studies in Fossil Botany,' p. 499.

† 'Ann. du Jardin Bot. Buitenzorg,' 1884.

‡ 'Phil. Trans.,' B, 1894, p. 544.

When there are two or three synangia they may still be separate, but they are crowded together, and in some cases may be more or less fused. Some cases at least of irregular quinquelocular synangia are due to the fusion of two original synangia, owing doubtless to the close proximity of their primordia. But if originally distinct synangia may become fused in their development, we have less difficulty in understanding how the three loculi of the synangium may be due to the fusion of three primitively separate sporangia.

The most important inference from this comparison is that the repeated dichotomy of the sporophylls of the family Psilotæ is an ancient feature. A real affinity with the Sphenophyllales is thereby rendered more probable.

In determining the affinities of the Phanerogams it is the custom to attach more importance to the characters of the flowers than to the vegetative characters, which are subject to many adaptive modifications. Bower has recently urged* the importance of the characters of the reproductive organs, and especially of the sporangia, in determining the affinities of the genera of Ferns. If we allow that the characters of the sporophylls and sporangia are entitled to more weight than vegetative characters in deciding the affinities of the Psilotæ, the family must be placed in the Sphenophyllales rather than in the Lycopodiales. The whorled arrangement of the leaves of the typical family Sphenophylleæ is the chief objection to this, but phyllotaxis is often a very variable character, and notably so in the Psilotæ, even though it must be admitted that the arrangement in whorls appears to have been a very constant feature in the Sphenophylleæ. It would seem, therefore, that although the character of the sporophylls, and especially the sporangiophores, justifies our including the Psilotæ in the class Sphenophyllales, they yet form a family rather remote from the Sphenophylleæ.

“On the Excretory Organs of *Amphioxus*.” By EDWIN S. GOODRICH, M.A., Fellow of Merton College, Oxford. Communicated by E. RAY LANKESTER, F.R.S. Received January 7,—Read January 23, 1902.

Some years ago, in 1890, Weiss and Boveri discovered excretory tubules in the pharyngeal region of *Amphioxus*.† Soon after Boveri

* ‘Phil. Trans.,’ B, vol. 192, 1900, p. 30.

† Weiss, F. E., “Excretory Tubules in *Amphioxus lanceolatus*,” ‘Quart. Jour. Micr. Sci.,’ vol. 31, 1890; Boveri, Th., “Ueber die Niere des *Amphioxus*,” ‘Sitz.-Ber. d. Ges. f. Morph. u. Phys. in München,’ Jahrg. 6, 1890.

published a detailed description and figures of these segmental kidneys.*

According to Boveri, each organ consists of a narrow ciliated tubule opening on the one hand into the atrium at the top of a secondary gill-bar, and on the other hand into the dorsal coelom by one or more funnels. Groups of peculiar cells, called "fadenzellen," are spread round each funnel, attached to the coelomic wall. These cells send each a long fine process passing towards the lip of the funnel across its opening.

During a visit to Naples this winter, I determined to re-examine these structures in *Amphioxus*, with a view to comparing them with the nephridia of certain Polychæte worms; and I am now able to state that the so-called "fadenzellen" are in reality typical *solenocytes*, such as I have described in the Glyceridæ, Phyllodocidæ, Alciopidæ, and Nephthyidæ.†

Hitherto the excretory cells, named solenocytes, have been known to occur only at the inner extremity of nephridia, which end blindly, having no direct communication with the coelom. Each solenocyte consists of a cell-body and nucleus situated at the distal free extremity of a delicate tube. The proximal end of the tube pierces the wall of the nephridial canal, and opens into its lumen. A single long flagellum, arising from the cell, works in the tube and projects into the canal.

Now I find that the excretory organ of an adult *Amphioxus* is formed of a ciliated tubule having an external but no internal opening. It is more or less branched, and the branches bear innumerable solenocytes with narrow thread-like tubes of remarkable length. At the distal end of each tube is situated the cell-body and nucleus, and inside it vibrates a flagellum which passes into the lumen of the excretory canal.

It may be concluded from these observations that, in their function segmental arrangement, and detailed histological structure, the excretory organs of *Amphioxus* are essentially identical with the nephridia of certain Polychæte worms.

* Boveri, Th., "Die Nierencanälchen des *Amphioxus*," 'Zool. Jahrb.,' vol. 5, 1892.

† Goodrich, E. S., "On the Nephridia of the Polychæta," 'Quart. Jour. Micr. Sci.,' Part 1, vol. 40, 1897; Part 2, vol. 41, 1898; Part 3, vol. 43, 1900.

"On the Causation of the so-called 'Peripheral Reflex Secretion' of the Pancreas. (Preliminary Communication.)" By W. M. BAYLISS, D.Sc., and ERNEST H. STARLING, M.D., F.R.S. Received January 22,—Read January 23, 1902.

(From the Physiological Laboratory, University College, London.)

Introduction.

It has long been known that the introduction of acid into the duodenum causes a flow of pancreatic juice, and it has been shown recently by Popielski, and by Wertheimer and Le Page, that this flow still occurs after nervous isolation of duodenum and pancreas.

Wertheimer also mentions that the flow can be excited by injection of acid into the jejunum, but not by introduction of acid into the lower part of the ileum.

These authors conclude that the secretion is a local reflex, the centres being situated in the scattered ganglia of the pancreas, or, in the case of the jejunum, in the ganglia of the solar plexus (Wertheimer).

Results.

The secretion excited by introduction of acid into the jejunum cannot be reflex, since it occurs after extirpation of the solar plexus and destruction of all the nervous filaments passing to the isolated loop of jejunum. It also occurs after intravenous injection of 0.01 gramme atropin sulphate. It must therefore be due to direct excitation of the gland cells by a substance or substances conveyed to the gland from the bowel by the blood stream.

The exciting substance is not acid. Wertheimer has shown that injection of 0.4 per cent. HCl into the blood stream has no excitatory influence on the pancreas.

The secretion must therefore be due to some substance produced in the intestinal mucous membrane under the influence of the acid, and carried thence by the blood stream to the gland.

This conclusion was at once confirmed by experiment.

When the mucous membrane of the jejunum or duodenum is exposed to the action of 0.4 per cent. HCl a body is produced which, when injected in minimal doses into the blood stream, produces a copious secretion of pancreatic juice. This body, which for the present we may term *secretin*, is associated with another body with a pronounced lowering effect on the blood pressure. The two bodies are not identical, since acid extracts of the lower end of the ileum produce the pressure-lowering effect, but have no excitatory influence on the pancreas.

The action of the acid is to split off the *secretin* from a precursor,

prosecretin, which is present in relatively large amounts in the duodenal mucous membrane, gradually diminishing as we descend the intestine.

The *prosecretin* is apparently slightly soluble in 0.9 per cent. NaCl solution. It has no influence on the pancreatic secretion.

The *secretin* may also be split off from its precursor by boiling this with 0.9 per cent. saline.

The acid extract may be boiled, neutralised, and filtered without losing its activity. The active filtrate contains some primary albumoses, which may be precipitated by addition of large excess of alcohol and ether, and have no action on the pancreas. The alcoholic ethereal filtrate contains only a small amount of organic material, but on evaporating to dryness and taking up the residue with water, the solution obtained is as active as the original infusion.

These results indicate that *secretin* is probably a body of very definite composition, and of small molecular weight. Dr. Osborne is at present engaged in an investigation of its chemical characters and identity.

A chemical sympathy between different organs has often been assumed, as, *e.g.*, between uterus and mammary glands, but we believe that this is the first case in which direct experimental proof has been afforded of such a relationship. It is probable, however, that this acid-duodenum-pancreas mechanism will prove to be but one of a whole class of similar mechanisms, the study of which should considerably increase our control of the various chemical functions of the body.

In the light of the results here noted, a revision of Pawlow's experiments is necessary. In none of these latter was a possible expulsion of acid from stomach into duodenum excluded. We have so far failed to obtain a secretory effect on the pancreas by stimulation of the vagi, and are therefore rather sceptical of the alleged presence in these nerves of secretomotor fibres to the pancreas. We are continuing our experiments on this and kindred points.

January 30, 1902.

Dr. W. T. BLANFORD, Vice-President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

- I. "The Chemical Origins of the Lines in Nova Persei." By Sir NORMAN LOCKYER, K.C.B., F.R.S.

- II. "The Specific Volumes of Oxygen and Nitrogen Vapour at the Boiling Point of Oxygen." By Professor J. DEWAR, F.R.S.
- III. "The Distribution of Magnetism as affected by Induced Currents in an Iron Cylinder when rotated in a Magnetic Field." By Professor ERNEST WILSON. Communicated by Sir WILLIAM PREECE, K.C.B., F.R.S.

"The Chemical Origins of the Lines in Nova Persei." By Sir NORMAN LOCKYER, K.C.B., F.R.S. Received December 23, 1901,—Read January 30, 1902.

In the first two papers* which I communicated to the Royal Society on the observations of Nova Persei at Kensington, a general description of the spectrum in its earliest stages was given, and also a statement as to the probable origins of some of the strongest lines, depending upon a comparison of the Nova Spectrum with those of α Cygni, α Canis Majoris, γ Orionis, &c.

A table showing the results of the detailed reduction and discussion of the bright lines in the best spectrum typical of the Nova in this early stage (that obtained with the 6-inch objective prism on February 25, when the star was nearly as bright as Capella), was undertaken later for inclusion in a general summary of the observations in course of preparation.

As various conclusions as to the chemical origin of some of the Nova lines have recently been published which differ widely from those arrived at from a careful study of the Kensington photographs, I think it desirable to publish the above-mentioned table at once. A detailed statement of the evidence on which these conclusions as to origin are based, will follow in the general summary before referred to.

That many of the stronger lines in the early spectra of Nova Aurigæ were chromospheric, has been pointed out by several observers, and all agree that there is little difference in the general aspect of the spectrum of Nova Aurigæ and that of Nova Persei at a corresponding stage of development. In confirmation of this it may be said that by means of direct comparison of the various Kensington photographs, the identity of most of the strong bright Nova bands with lines of α Cygni and the chromosphere—already known to be due to enhanced lines of certain metals—has been established.

With regard to the stellar relations of the Nova, I may state that in my note of February 28,† the agreement in position of its lines with

* 'Roy. Soc. Proc.,' vol. 68, p. 119 and p. 142.

† 'Roy. Soc. Proc.,' vol. 68, p. 121.

Comparison Table of Bright Lines in the Spectra of Nova Persei and Nova Aurigæ with α Cygni Lines, Chromospheric Lines, and the Enhanced Lines of some of the Metals.

Nova Persei (Kensington).		Nova Aurigæ.		α Cygni.	Chromo- sphere (1898 Eclipse).	Enhanced lines.		Remarks.
λ of middle of line.	Description of line.	Campbell.	Vogel.			λ .	Sub- stance.	
3836	Very broad, weak	—	3835	—	3835·6	—	—	H η .
3889	Very broad. ? Narrow reversal in centre	—	3889	3889·1	3889·1	—	—	H ζ .
3934	Broad, rather weak. Fine narrow reversal in centre	—	3934	3933·8	3933·83	3933·83	pCa	K (Ca).
3969	Very broad and fairly strong; shows fine narrow reversal in position of H (Ca) and possibly another weaker one in position of H ϵ	—	3969	{ 3968·6 3970·2	{ 3968·63 3970·25	3968·63	pCa	H (Ca) + H ϵ .
3982	{ All these are very weak diffuse lines which can just be seen with the unaided eye, but which will not bear any mag- nification, and their wave- lengths can only be roughly determined	—	—	4002·7	—	4002·77	pFe	Strongest enhanced line of Ni.
4003		—	—	4028·5	4028·5	4028·50	pTi	
4019		—	4067	4067·3	4067·3	4067·30	pNi	
4029		—	—	—	—	—	—	
4067	{	4082	—	4077·9	4077·9	4077·88	pSr	H δ .
		4095	—	4101·8	4101·8	—	—	
		4102	—	—	—	—	—	
4102		4108	—	—	—	—	—	

Comparison Table—continued.

Nova Persei (Kensington).		Nova Aurigæ.		α Cygni.	Chromo- sphere (1868 Eclipsæ).	Enhanced lines.		Remarks.
λ of middle of line.	Description of line.	Campbell.	Vogel.			λ.	Sub- stance.	
4128	Very broad and merging into Hδ	4126	4125	{ 4128·1 4131·1	—	{ 4128·1 4131·1	pSi pSi	Mean λ of enhanced double = 4176·23.
4175	Broad and fairly strong	4166	4158	{ 4163·8 4173·5	4163·8 4173·5	4163·82 4173·52	pTi pFe	
		4180	4176	{ 4179·0	4179·0	4178·95	pFe	
		4209						
4232	Broad, but not very strong	4227	4230	4233·3	4233·3	4233·25	pFe	Strongest spark line of Sc.
4247	Comparatively narrow, but merging into the broader 4232 line, slightly stronger than the latter	4236	—	4247·2	4247·0	4247·00	pSc	
4262	Narrow, but fairly strong	4267	4262	4262·2	—	4262·15	pCr	Mean λ of enhanced Fe double = 4300. Possibly due to Sc 4314·25, one of the strongest spark lines.
			4288	4290·4	4290·2	4290·38	pTi	
4300	Broad line, fairly strong	4296	—	{ 4296·7 4300·2	4296·7 4300·2	4296·65 4300·21	pFe pTi	
4314	Comparatively narrow, merging into 4300	4316	4315	{ 4303·3 4315·1	4303·0 { 4314·0 4315·1	4303·34 4315·14	pFe pTi	
4341	Broad and very strong. Doubt- ful reversal in centre	4331·3 4340·6 4347·8	4341	4340·7	4340·7	—	—	Hγ.
		4355	—	—	4355	4374·90	pTi	
		4375	—	4374·9	4374·0	4874·90	pTi	
		4385	4383	4385·5	4385·5	4885·55	pFe	

	Fairly broad, rather weak. Broad and moderately strong ...	— 4419	— 4417	4411·2 { 4417·0 4417·9	4411·2 4417·9	pTi pTi
4482	Weak and rather narrow.	4436	4435	4434·4	4435·5	pTi
4483	Fairly broad, but weak —	4445	4445	4444·0	4444·0	pTi
4484	Rather narrow, weak.	4471	4473	4468·7	4468·7	pTi
4485	" " and weak	4481	4480	4481·3	4481·3	pMg
4486	" " fairly strong.....	4490	4495	4489·0	4489·3	pFe
4487	" " " "	4502	4507	4491·6	4491·6	pFe
4488	" " " "	to	4520	4501·5	4501·5	pTi
4489	" " " "	4534	4530	4508·5	4508·5	pFe
4490	" " weak.....	4549	—	4515·5	4515·5	pFe
4491	" " moderately strong	4554	4557	4520·4	4520·4	pFe
4492	" " " "	4559	—	4522·7	4522·7	pFe
4493	" " " "	4564	—	4534·1	4534·1	pTi
4494	" " " "	4570	—	4549·8	4549·7	pFe
4495	" " " "	4576	—	4556·1	4556·1	pFe
4496	" " but fairly strong.	4586	—	4558·8	4558·8	pCr
4497	Broad and fairly strong	4630	—	4563·9	4563·9	pTi
4498	" " rather weak	4669	—	4572·2	4572·2	pTi
4499	" " weak	4707	—	4576·5	4576·5	pFe
4500	" " " "	4737	—	4584·0	4584·0	pFe
4501	" " " "	4774	—	4629·6	4629·5	pFe
4502	Fairly broad, weak	—	—	4824·3	4824·3	pCr
4503	Very broad and strong	{ 4551·2 4561·6 4569·9 }	4862	4861·5	—	Hβ.

Comparison Table—*continued*.

Nova Persei (Kensington).		Nova Aurigæ.		α Cygni.	Chromo- sphere (1898 Eclipse).	Enhanced lines.		Remarks.
λ of middle of line.	Description of line.	Campbell.	Vogel.			λ.	Sub- stance.	
4924	Broad and strong.....	{ 4913 4923 4929 4969 }	{ 4923 4925 }	4924.1	4924.1	4924.11	pFe	
5019	" "	{ 5007 5018 5095 5142 }	5016	5018.6	5018.6	5018.63	pFe	
5169	Broad and very strong.....	{ 5150 5169 5176 5200 }	5167	5169.1	5169.1	{ 5169.07 5169.23 }	pFe	Certainly not the δ- group of magne- sium.
*5224	Narrow and weak.	5234	523	--	5234.8			
*5273	" "	{ 5276 5285 }	528	--	{ 5275.1 to 5276.2 }	5276.17	pFe	
5316	Moderately broad and strong....	{ 5318 5329 5379 }	5317	5316.8	5316.8	5316.79	pFe	
*5420	Weak and rather narrow.	5454	--	--				
*5475	Weak and narrow.	5535	--	--	5527.6	5527.03	pSc	By far the strongest spark line of Sc in the visual region.
*5530	Fairly strong and broad.....							

*5575	"	"	"	5575 5584 5630 5685 5761 5841 5885 5896					
*5686	"	"	"	5686†					
*5893	"	"	"		—	{ 5890.2 5896.2 }	—	—	D(Na).

† As given by Vogel (probably 5896).

Note.—The lines marked* cannot be seen in the photographs taken with the 6-inch prismatic camera, and their positions have been reduced from the spectrum photographed with the 30-inch reflector with a two-prism slit spectroscope.

those of α Cygni was pointed out. Later, Mr. McClean* confirmed this, and further stated that the bright lines of η Argus corresponded with the dark lines of the Nova; while later still, Sir David Gill† has shown the similarity between the spectra of η Argus and Nova Aurigæ. Hence we are led to the conclusion that the temperatures reached in the outbursts of both these Novæ were not greatly different from those of the stars named, and that in η Argus there are constant conditions which are similar to those temporary conditions which produce the appearance of Novæ.

In the table the lines of Nova Persei are compared with those recorded by Campbell‡ and Vogel§ in Nova Aurigæ.

The probably corresponding lines of α Cygni, chromospheric lines, and enhanced lines, are also given in separate columns.

The reduction to wave-lengths and discussion of the lines in relation to those of other celestial and terrestrial spectra, has been undertaken by Mr. F. E. Baxandall, to whom my best thanks are due.

“The Specific Volumes of Oxygen and Nitrogen Vapour at the Boiling-point of Oxygen.” By JAMES DEWAR, M.A., D.Sc., LL.D., F.R.S. Received January 21,—Read January 30, 1902.

In my paper on “The Boiling-point of Liquid Hydrogen determined by Hydrogen and Helium Gas Thermometers”|| it was pointed out that a constant-volume gas-thermometer filled with oxygen gas, having a pressure at 0° C. of about 800 mm., gave a very accurate value of the boiling-point of liquid oxygen. As it seemed advisable to confirm this result indirectly, an attempt was made to determine the vapour density of oxygen at its boiling-point by direct weighing, the intention being, if the experimental results proved at all encouraging, to repeat the work on a larger scale and with greater precautions. As at present there is no likelihood of my being able to undertake the more accurate determinations, the results of the preliminary enquiry are presented to the Society. They give in any case, with considerable accuracy, the Specific Volumes which have never been directly determined.

In order to obviate any question of the buoyancy of the air, two flasks A and B of as nearly as possible the same air displacement were counterpoised on an (Erting balance. The B flask remained per-

* ‘M.N., R.A.S.,’ vol. 61, p. 387.

† ‘Roy. Soc. Proc.,’ vol. 68, p. 457.

‡ ‘Ast. and Ast. Phys.,’ vol. 11, p. 808.

§ ‘Ast. and Ast. Phys.,’ vol. 12, p. 912.

|| ‘Roy. Soc. Proc.,’ vol. 68, 1901.

manently on one scale of the balance during all the weighings, while the A flask was weighed, either exhausted or filled with oxygen (or nitrogen), under various circumstances according as the experiments required.

As the flask cooled in the liquid oxygen or air had to stand an internal pressure of from three to four atmospheres, it was considered expedient to select a spherical vessel of about 300 c.c. capacity, to which was sealed a narrow tube having a very carefully ground stopcock at its end. Preliminary experiments were made to determine the change of volume of the flask when subjected to internal pressure. The flask, filled with air at five atmospheres pressure, was left for 24 hours with the stopcock closed, without showing any leakage. To determine the effect of pressure on the capacity of the flask, it was filled with water under one atmosphere pressure, and again with water under three to four atmospheres pressure (which included the range of the observations) and the weights noted. From these the coefficient of expansion of the flask was found to be 0.000306 per atmosphere excess of internal over external pressure. The temperature coefficient of expansion of glass (cubical) was taken to be 0.000025, which for a variation of temperature of some 200° altered the capacity of the flask by about 1.5 c.c. The content of the flask up to a fixed mark on the neck below the stopcock was determined to be 315.973 c.c. at 17° C.; the content between the mark and the stopcock was determined both by measurement and by the weight of mercury it contained, and was found to be 0.127 c.c.

Before each experiment the A flask filled with the gas under observation was exhausted to a pressure of from 2 to 4 mm. of mercury (which was afterwards involved as a correction in reducing the observations) and weighed, the weight *a* which had to be added to its scale pan to balance the B flask being noted. The A flask was then filled with carefully purified oxygen (or nitrogen), and the temperature of the flask and contents (still in communication with the gas reservoir) was lowered by immersing it up to the mark in liquid oxygen (or air) until the gas ceased passing into the flask, and the pressure was finally equalised to that of the atmosphere. This is really the most important part of the manipulation, as the accuracy mainly depends on giving sufficient time to the cooling, while at the same time taking care to avoid any excess of pressure that would necessarily lead to liquid condensation on the glass surface. During the rapid inrush of gas, it is advisable to keep the pressure well below that of the barometer at the time, and to finally adjust the pressure at the end of the absorption. The vessel containing the liquid oxygen had a cardboard cover which crossed the neck of the flask at the mark, thereby preventing the cooled vapour of the oxygen from freezing the stopcock. The remaining portion of the tube between the mark and the stopcock, namely,

0.127 c.c., was therefore approximately at the temperature of the room. When the temperature of the flask and its contents became stationary the stopcock was closed, the flask removed, and after heating up to the temperature of the room, weighed against the B flask and any further necessary counterpoise b . The weight of oxygen vapour in the flask at its boiling-point was thus equal to $a + b$, subject to the corrections which have been indicated.

To the five sources of error or correction indicated above, there remains to be added that due to additional buoyancy of the air during the weighing of the flask, when filled with oxygen at its boiling-point, for these weighings were made at the temperature of the room, which would cause a rise of pressure in the flask and therefore expansion of its volume. But this error was so small that it could safely be neglected.

As the intention was not only to ascertain the density of oxygen and nitrogen at their respective boiling-points under atmospheric pressure, but also under diminished pressure, experiments were made with nitrogen at ordinary temperatures and at pressures varying from about one-sixth of an atmosphere to ordinary pressures, in order to find the range of variation in the results with the 316 c.c. flask to be used in the subsequent low-temperature experiments. In the following table are details of these observations, and the results reduced according to the ordinary gaseous laws.

Table I.—Density of Nitrogen, ordinary Temperature.

No.	p_1 .	T_1 .	p .	a .	b .	V.	d .	d_0 .
	mm.	°	mm.	gramme	gramme			
1	742.0	15.5	742.0	0.37	-0.0022	315.973	0.001168	0.001265
2	742.0	15.5	742.0	0.3668	-0.001	315.973	0.001162	0.001258
3	742.0	16.0	742.0	0.367	-0.0015	315.973	0.001161	0.001259
4	751.0	16.5	281.3	0.371	-0.2328	315.913	0.000442	0.001265
5	751.0	16.5	284.2	0.371	-0.231	315.913	0.000447	0.001269
6	751.0	17.0	268.5	0.371	-0.239	315.911	0.000422	0.001269
7	753.0	17.5	176.2	0.3705	-0.2850	315.899	0.000275	0.001262

where p_1 = barometric pressure at the time of observation,

T_1 = temperature of the room at the time of observation,

p = pressure of the nitrogen vapour in the flask during the experiment,

a, b = as defined before,

V = the volume of the flask corrected for temperature and compression,

d = the calculated density at T_1° and pressure p ,

d_0 = the value of d reduced to 0° and 760 mm. by the ordinary gaseous laws.

In reducing these observations the following corrections were involved: a correction of 0·0015 gramme, due to imperfect exhaustion of the flask A while being weighed as empty; the correction due to the neck of the flask between the mark and the stopcock was negligible; the volume of the flask when corrected for temperature was 315·973 c.c., and when it was necessary to correct further for excess of external over internal pressure, the values were as given in Column V; in the last four experiments this correction varied from about 0·059 to 0·073 c.c.

The first three experiments give a mean value of 1·260 grammes, at standard temperature and pressure, as the weight of a litre of nitrogen. This is about a quarter per cent. higher than the accepted value of 1·257. The extreme variation in the individual experiments is about half a per cent. The average value of the results under about one-third of an atmosphere is 1·266 grammes; the tendency under the low pressures being to make the density half a per cent. higher. Considering that in the actual low-temperature experiments the mass of gas to be weighed would be at least three times greater, it was inferred that in spite of difficulties of manipulation and corrections the results might be anticipated to lie within a half per cent. of the true value.

The following table gives the results of six experiments made on the density of the vapour of oxygen at its boiling-point taken as 90°·5

Table II.—Density of Oxygen Vapour at its Boiling-point.

No.	$p = p_1$.	T_1 .	a .	b .	d .	v .
	mm.		gramme.	gramme.		
1	775·5	15°·5	0·388	1·0225	0·004402	227·15
2	770·3	16°·5	0·3865	1·015	0·004403	227·07
3	771·0	17°·5	0·3875	1·0245	0·004432	225·60
4	771·8	17°·5	0·3875	1·0260	0·004432	225·59
5	775·0	16°·0	0·388	1·028	0·004422	226·12
6	774·2	16°·5	0·3875	1·028	0·004425	225·97

absolute, and under atmospheric pressure, where p , p_1 , T_1 , a , b , are the same symbols as used in the previous table, d is the calculated density at 90°·5 absolute and 760 mm., and v is the specific volume $1/d$.

In the above reductions the following corrections were involved: 0·002 gramme due to imperfect exhaustion of A flask while being weighed as empty; 0·0002 gramme due to the contents of the neck of the flask between the mark and the stopcock not being at 90°·5; the volume of the flask up to the mark contracted at 90°·5 to 314·398 c.c.; no correction was necessary for compression.

Thus the mean weight of one litre of oxygen vapour at 760 mm. and 90°·5 absolute is 4·420 grammes, and the specific volume is

226.25 c.c. If the first two experiments are eliminated on the assumption that the proper equilibrium of temperature had not been attained, the average weight per litre would become 4.428 grammes, and the specific volume 225.82.

Taking Regnault's density of oxygen at 0° and 760 mm., the density at 90°·5 in the ordinary way would be 0.0043137, and the specific volume 231.82 c.c. Thus the volume given by the ordinary gaseous laws is 1.0246 times the average observed volume; or we may put it that pv is diminished at the boiling-point of oxygen by 2.46 per cent. Again, while the ratio of the absolute temperatures is 3.017, the ratio of the densities is 3.091.

Behn* has determined by an indirect method the specific volume of oxygen, and finds the value 358, which is nearly 60 per cent. greater than the volume found by the direct method. The mode of procedure he adopts is to ascertain directly three quantities out of four in the ordinary thermodynamic equation correlating latent heat, temperature, increment of pressure to temperature, and specific volume, thereby deducing the unknown quantity. Now of the three experimental values required, one, viz., increment of pressure to temperature, can only be got by calculation from the vapour-pressure curve, and much depends upon the accuracy of this value. Accepting Estreicher's vapour pressures for liquid oxygen below its boiling-point as the most reliable, a Gibbs equation gives the increment per degree near 7.40 mm. pressure as 78.67 mm. mercury pressure or 106.93 grammes per square centimetre. This value, taken along with the latent heat found by Behn, and the boiling-point, gives, when inserted in the thermodynamic equation, a specific volume of 223.55, which is within less than 2 per cent. of the value found by the direct-density determinations.†

Further experiments were made on oxygen vapour at 90°·5 and under reduced pressures. These experiments and their results are given in the following table. The same symbols are used as in the preceding tables, except that d is the calculated density at 90°·5 absolute and pressure p , and v is $1/d$. The corrected volume of the flask is entered under V .

* 'Ann. der Physik,' vol. 1, 1900, "Sublimationswärme der Kohlensäure und die Verdampfungswärme der Luft."

† In the same way Behn's specific volume of carbonic acid would be contradictory of my proof that a constant-volume gas-thermometer filled with carbonic acid at about atmospheric pressure gives a very accurate value of its own boiling-point. Assuming the ordinary gaseous laws, the specific volume ought to be 361.6 instead of 423 given by Behn. Now my value of the increment of pressure to temperature at the boiling-point is 62.84 mm. mercury pressure, and this, along with the values used by Behn in the thermodynamic equation, gives 363 as the specific volume. This comes much nearer the anticipated value of the constant.

Table III.—Density of Oxygen Vapour at its Boiling-point under Diminished Pressure.

No.	p_1 .	T_1 .	p .	a .	b .	V .	d .	v .
	mm.	°	mm.	gramme	gramme			
1	741·8	18·5	287·0	0·3695	0·1315	314·340	0·001588	625·74
2	741·8	18·5	281·5	0·3695	0·1345	314·339	0·001607	622·01
3	746·5	20·0	279·0	0·3690	0·1290	314·338	0·001588	629·41
4	746·5	20·0	310·2	0·3690	0·1890	314·342	0·001776	562·95
5	746·5	19·0	159·4	0·3690	0·0850	314·323	0·000907	1101·46

In reducing these observations the following corrections were involved: a correction of 0·0014 gramme to 0·0015 gramme due to imperfect exhaustion of the A flask while being weighed as empty; the correction due to the neck of the flask between the mark and the stopcock not being at 90°·5 amounted to 0·00003, 0·00007 gramme and was practically negligible; the volume of the flask, which, when corrected for temperature, was 314·398 c.c., had to be further corrected for excess of external pressure over internal pressure by amounts varying from 0·0551 c.c. to 0·0742 c.c. If the first three experiments are averaged (the pressures being so near), the weight of a litre of oxygen at 90°·5 absolute under a pressure of 282·5 mm. would be 1·5982 grammes. The ratio of this density to the value previously found for one atmosphere pressure, viz., 4·42 grammes, is 2·765, and the ratio of the pressures is 2·690. It appears that the ratio of the change of density of the vapour of oxygen at 90°·5 absolute, under variable pressure, is greater than the ratio of the change of pressure. It is clear, however, that it would be necessary to work upon a larger scale in order to get satisfactory vapour densities at low temperatures under pressures below that of the atmosphere.

The following table gives the observations on the density of nitrogen vapour at the boiling-points of liquid oxygen and liquid air respectively; the first two were made in oxygen, the last four in air. The symbols used are the same as before, except that d is the calculated density at T° absolute and 760 mm. and v is $1/d$.

Table IV.—Density of Nitrogen Vapour at the Boiling-point of Oxygen.

No.	$p = p_1$.	T_1 .	a .	b .	d .	v .	T .
	mm.	°	gramme.	gramme.			
1	771·8	17·5	0·3875	0·8555	0·0039021	256·27	(90°·5)
2	773·0	17·0	0·385	0·8555	0·003885	257·39	(90°·5)
3	777·2	16·0	0·3885	0·9575	0·004192	238·53	84·05
4	777·3	16·5	0·3885	0·9500	0·004168	239·90	84·54
5	777·3	16·5	0·3885	0·942	0·004143	241·34	85·04
6	777·3	16·5	0·3845	0·9235	0·004073	245·49	86·50

In reducing these observations, the following corrections were involved: a correction of 0.002 gramme due to imperfect exhaustion of the A flask, while being weighed as empty; the correction due to the neck of the flask between the mark and the stopcock not being at temperature T amounted to 0.00014 gramme and was practically negligible; the volume of the flask, which only required to be corrected for temperature, was 314.398 c.c.

Experiments 1, 2 were made with liquid oxygen taken to be at temperature $90^{\circ}5$ absolute. The experiments 3, 4, 5, 6 were made in one and the same sample of liquid air, with rising temperature. For these temperatures, obviously only a few degrees below the boiling-point of oxygen, the ordinary gaseous laws may be held to apply, in order to determine their values. Thus we may employ the formula

$$T = v \frac{90.5}{256.833},$$

where 256.833 is the mean of the volumes of Nos. 1, 2, to get the temperature of the last four experiments. The values thence obtained are entered in column T .

The first two experiments made with liquid oxygen give a ratio of the nitrogen densities from my own values of 3.088, the absolute temperature ratio being 3.017; my values for the ratio of the oxygen densities for the same range of temperature being 3.091 as previously deduced. We may safely assume that if the density of nitrogen were observed at its boiling-point it would deviate as much from the ordinary gaseous laws as oxygen. Further, the specific volume of nitrogen at its boiling-point of 78° absolute, would from the above formula be 221.3 as compared with 226.2, the similar value found for oxygen.

The general inference to be drawn from these preliminary experiments is that reliable vapour densities may be determined at very low temperatures. There seems to be no reason why the vapour density of hydrogen at its boiling-point should not be accurately ascertained; only, as in this case the internal pressure in the weighing flask would amount to nearly 15 atmospheres, it would be advisable to construct the flask of some metal or alloy. A flask of the size used in the oxygen experiments filled with the vapour of hydrogen at its boiling-point would be equivalent in weight to between 4 and 5 litres of hydrogen at the ordinary temperature and pressure, and such an amount of material ought to give density results at the boiling-point of hydrogen of considerable exactness, notwithstanding the great manipulative difficulties that would necessarily be involved in the execution of such a determination at 21° absolute.

"A Short Description of the Culicidæ of India, with Descriptions of New Species of Anopheles." By FRED. V. THEOBALD, M.A., F.E.S. Communicated by E. RAY LANKESTER, F.R.S. Received December 10, 1901,—Read January 23, 1902.

[PLATE 5.]

The following account has been drawn up at Professor Lankester's request with a view to facilitating the work of investigation of malaria in British India. It includes all the known species of Culicidæ found in continental India. Singalese and Malayan species are not recorded, but probably several of them will be found in India.

The species previously described are here only referred to briefly, sufficient characters only being given to enable the identity of the Indian species being made; for this purpose tables have also been drawn up. Full descriptions, references, &c., will be found in the "Monograph of the Culicidæ of the World," published by the Trustees of the British Museum (Natural History).

The following genera are recorded from India:—*Anopheles*, Meigen; *Toxorhynchites*, Theobald; *Mucidus*, Theobald; *Stegomyia*, Theobald; *Armigeres*, Theobald; *Culex*, L.; *Tæniorhynchus*, Arribalzaga; *Panoplites*, Theobald; *Ædeomyia*, Theobald; and *Uranotenia*, Arribalzaga. In all forty-five species of Culicidæ are known, and one *Corethra* (*C. Asiatica*), which is a non-piercing-mouthed genus, scarcely tenable in the family Culicidæ.

This number will probably be doubled in a few years. At present we have no records of *Sabethes*, *Psorophora*, *Megarhinus*, *Trichoprosopon*, *Janthinosoma*, *Ædes*, and several other genera. *Sabethes*, *Trichoprosopon*, and others occur in woods and forests. At present we seem mainly to have received collections of household forms and species found in the immediate neighbourhood of man, from the Indian continent.

It is necessary that other genera than *Anopheles*, *Stegomyia*, and *Culex* should be experimented with in connection with malarial fever, yellow fever, Filariasis, &c., and that *all* species of the various genera should be dealt with. There is no reason, if only *certain species* of *Anopheles* act as carriers of malarial parasites, why some species of *Culex*, *Stegomyia*, *Panoplites*, &c., should not act in the same way, whilst others of them do not. For this reason, all the Indian Culicidæ are here recorded for reference. Filariasis is carried not only by *Culex*, but we know also by certain *Anopheles* and *Panoplites*, and possibly further experiments will show the same for malaria and other diseases.

It will be noticed that five European species occur in India (*fasciata*, *mimeticus*, *spathipulpis*, *cantans*, and *nigripes*).

No doubt others, as *C. pipiens*, *A. superpictus*, &c., will be found on further search being made. The most interesting is the presence of *C. cantans* in South India.

Genus ANOPHELES Meigen.

Table of Indian Anopheles.

A. Metatarsi and tarsi banded.

a. Posterior tarsi pure white.

β. Last three hind tarsi and most of the first white.

γ. Abdomen densely scaly. *pulcherrimus*, n. sp.

γγ. Abdomen not densely scaly.

Costa with three small white spots; wings very dark; fringe at apex black. *fuliginosus*, Giles.

Costa with seven alternate black and yellow spots; fore metatarsus banded in middle. Legs slightly spotted.

Jamesii, Theobald.

ββ. Last two hind tarsi white.

Wings jet black, with five white costal spots and an apical spot. *Theobaldi*, Giles.

βββ. Last hind tarsus white.

Legs speckled and banded. *maculatus*, Theobald.

αα. Posterior tarsi not pure white.

δ. Costa with two pale spots.

i. Palpi densely scaled and banded.

Cross-veins close together; base of 1st sub-marginal cell not under apex of the sub-costal, pale costal spots small. *sinensis*, Wiedemann.

Cross-veins far apart; base of 1st sub-marginal distant from end of sub-costal; apical fringe pale.

Sub-sp. *annularis*, Van der Wulp.

Cross-veins as above, but base of 1st sub-marginal cell lies just under the end of the sub-costal.

Indiensis, Theobald.

Near *annularis*, but the wing-scales less elongated, and a black apical fringe and pale patch at end of lower branch of 5th vein. *nigerrimus*, Giles.

ii. Palpi densely scaled, but unbanded.

barbirostris, Van der Wulp.

δδ. Costa with more than two pale spots.

ε. Legs with basal pale bands. *gigas*, Giles.

εε. Legs with apical pale bands, legs speckled.

metaboles, n. sp.

εεε. Legs with apical and basal banding. *Rossii*, Giles.

εεεε. Legs unbanded; wing fringe with three pale spots.

Palpi with two narrow white rings and a minute white tip. *culicifacies*, Giles = *Listoni*, Giles = *Indica*, Theobald.

Wing fringe with more than three pale spots.

Palpi with two broad apical white bands, and a third narrow one. *Christophersi*, n. sp.

Palpi with a black tip. *Turkhudi*, Liston.

Legs speckled and banded, abdomen densely scaled, two eye-like thoracic spots. *Kochii*, Dönitz.

ii. Costa uniformly colored.

Femora with a white band. *Lindesayii*, Giles.

Anopheles pulcherrimus. n. sp.

Thorax ashy-brown with frosty grey and very pale ochraceous scales, unadorned, except for two narrow dark lines on the posterior half. Abdomen densely covered with frosty and pale ochraceous scales, darker basal bands and distinct lateral apical tufts of black scales; venter and sides mostly white scaled. Wings with the fringe mostly pale, with seven more or less distinct dark patches; costal border with four large black spots, and a few small ones on the yellow-scaled veins. Legs ashy-grey, the fore and mid tarsi and tibiae with apical white bands; in the hind legs, all the tarsi white, except the base of the first, which is black; a large white spot, nearly a band, near the apex of the femora. Ungues equal and simple.

♀ Head densely clothed with broad upright forked scales, creamy white in front, rich ochraceous behind; with a dense tuft of white hairs projecting forwards; antennae dark brown, with tufts of white scales on the joints, verticillate hairs white; palpi densely scaly, covered with brown scales and four prominent white rings, the broadest being the apical one.

Thorax brown to ashy-brown, covered with broad, flat, spindle-shaped grey scales, giving it a frosty appearance. Some of these scales have a slight ochraceous tinge in some lights, three distinct tufts of long and some short white scales projecting forwards over the head; thoracic hairs pale golden; scutellum brown, with similar scales to the mesonotum, and pale golden border-bristles may be seen when held in one direction, black when held in another; metanotum brown; pleurae densely white scaled. (When held in certain lights two dark lines show on the posterior part of the mesonotum, due to two dark lines on the denuded surface.) Abdomen black, densely clothed with flat scales, the base of each segment nude and thus black, then follows a row of white scales, the remainder rich ochraceous; at the sides of the apical end of each segment is a tuft of rather long flat black scales; border-bristles pale; venter black, with flat white scales. Legs with

pale bands ; fore legs with the femora and tibiæ grey beneath, with a few brown scales scattered above, metatarsi and first two tarsi with broad apical white bands, last two tarsi and bases of the others dark-brown, unguis equal and simple ; mid legs much the same, but the femora dark towards the apex, with a large white spot near the apex, which is black ; apex of tibiæ, metatarsi and first two tarsals white, last two deep brown, unguis equal and simple. In the hind legs the femora and tibiæ are much as in the mid, the apex of the metatarsus is white, and the whole of the tarsi except a narrow black band at the base of the first joint ; unguis small, equal and simple.

Wings (Plate 5, fig. 2) with the veins mostly clothed with pale creamy scales, with four large black costal spots and two small basal ones, the apical, second and fourth black costal spots extend evenly on to the first long vein, the third has a large median and a small dark spot at each end ; there are also small black spots on the veins as follows :—one at the base of the first fork-cell, one at the base and another at the apex of the third long vein ; two on each branch, and three on the stem of the second fork-cell ; three on the upper branch and one at the apex of the lower branch of the fifth long vein, and another at its base ; three on the sixth ; the fringe is mostly dark with seven pale patches ; halteres pale ochraceous.

Length.—4·5 mm.

Habitat.—Lahore, India.

Observations, described from three specimens sent by Captain James, I.M.S., and Drs. Christophers and Stephens.—It is a very well defined and beautiful species, closely related to *A. Kochii*, Dönitz, but quite easily separated from it by the white hind tarsi, the absence of thoracic ocelli, and by the wing ornamentation.

The ♂ is not known.

The type has been deposited in the British Museum (Nat. Hist.) collection. The name was proposed by the collectors.

Anopheles fuliginosus. (Giles.)

A. Jamesii, Liston.

'Handbook of Gnats,' p. 160 (1900), Giles ; 'Mono Culicid,' 1, p. 132 (1901), Theobald ; 'Ind. Med. Gaz.,' Dec., 1901, p. 441 (= *Jamesii*), Liston.

Thorax slaty black, with scattered flat spindle-shaped scales ; legs dark brownish-black, a pale band near the apex of the femora, a white apical band to the metatarsi and first two fore tarsi ; the last three and apex of the first hind tarsi white ; the legs never spotted.

The costa of the wing is densely black scaled, with four white more or less prominent spots, and often two smaller ones, the first long vein with either five or six white spots; veins mostly dark scaled, and the fringe has a pale spot where each vein joins the border.

Length.—3·5 to 4·2 mm.

Localities.—Madras; Quilon; Punjaub; Calcutta; Behar; Bengal; Dacca; Mukerian; Hosiarpur; Sambalpur (C.P.); Ellichpur.

Observations.—Very closely related to *A. argyrotarsis*, Desv., which Lieutenant-Colonel Giles informs me also occurs in India. A light variety, described as var. *pallida* ('Mono. Culic.', 1, 134), occurs in Madras. Captain Liston has described this insect as a new species, *A. Jamesii*; had this insect been new, the name could not stand, as I had previously employed it for the following species.

Anopheles Jamesii. (Theobald.)

'Mono. Culicid.', 1, 134 (1901).

Thorax brown, with ashy-grey and creamy-curved scales. Abdomen dark brown, with golden hairs and golden-brown scales on the last few segments. Palpi black, with snow-white rings, and white apical joint. Legs brown; fore femora and tibiae more or less spotted; fore metatarsus apically banded and also with an indistinct median band, first and second tarsi apically banded, and in the mid legs the metatarsus and first two tarsi apically banded; hind legs with femora and tibiae with an apical white spot, last three tarsi white, and also the apex of the first. Costa with four large and two small dark spots.

Length.—3 to 3·5 mm.

Habitat.—Quilon, Travancore; Ceylon; Deccan.

Observations.—A small species, easily told from the other Indian species of this group by the faint mid banding of the fore metatarsi and the more or less speckled femora and tibiae. Closely related to *A. fuliginosus*, but may be easily told from it by the above characters.

It is much smaller than the former species. I have only seen it from the south of India and Deccan.

The male at present is unknown.

Anopheles Theobaldi. (Giles.)

'Ento. Mo. Mag.', p. 198 (1901).

Thorax sooty-black, with broad lines of white scales, two median and two lateral. Abdomen sooty, nude, with brownish hairs. Palpi black, with white apical joint and three small white rings. Legs brindled with white scales and a large sub-apical white patch on the femora; tibiae and first tarsal joints white ringed, especially on the mid legs, last two hind tarsi all white. Wings jet black, with the costa

interrupted by five white spots and an apical spot; fringe with pale patches where the veins join the borders; veins mainly dark scaled, but with pale areas.

Length.—3·5 to 4 mm.

Habitat.—Ellichpur, Behars; Shahjahanpur, N.W.P.

Observations.—This species can at once be told by the last *two* tarsi of the hind legs only being white.

The female only known.

Anopheles maculatus. (Theobald.)

‘Mono. Culicid.,’ 1, p. 171 (1901).

Thorax slaty-grey, with darker longitudinal lines and snowy-white scales; abdomen black, with yellowish hairs, very dense apically, giving the apex a dense yellow tinge. Palpi black scaled, apical joint white, also base and apex of the penultimate and apex of the others. Legs with femora, tibiæ and metatarsi banded with pale yellow; fore and mid tarsi with narrow yellow bands, hind tarsi with broad white ones, last joint pure white. Wings with four large and two small black costal spots; veins mostly pale scaled with small black spots; fringe with pale spots where the veins join the border. Male palpi brown, apical joint dilated, white on one side and a small white spot on the other side near the base; apex of penultimate joint white, and another pale ring towards the base of the palp.

Length.—3·5 to 4 mm.

Habitat.—Lahore; Punjab (Christophers); Hong Kong (Rees).

Observations.—Described from specimens from Hong Kong. Dr. Christophers also sends specimens. Easily distinguished by the last hind tarsal only being white. Captain James, I.M.S., sent me notes on the larvæ of this species from Hong Kong, the larvæ being found in the shallow pools of marshy ground on the granite soil near Hong Kong.

Anopheles Sinensis. (Wiedemann.)

‘Aussereurop. Zweiflög. Insek.,’ p. 547 (1818); ‘Mono. Culicid.,’ 1, p. 137 (1901), Theobald.

Thorax slaty-grey, with purplish longitudinal stripes and narrow pale golden scales and minute dark specks. Abdomen brownish-black and testaceous, with long golden hairs and traces of paler apical borders. Legs brown, paler below; tibiæ and tarsi with either apical or apical and basal pale bands, except the last two of the fore and mid legs. Palpi densely scaled with dark brown scales, apex white, and with other white bands. Costa dark brown, with two large yellow spots.

Length.—5 mm.

Habitat.—Calcutta ; Madras ; Formosa ; China.

Observations.—This is the type of a group of closely-related mosquitoes which differ mainly in the position of the cross-veins and fork cells. These I have separated as sub-species. The following occur in India :—

Sub-species *annularis*. (Van der Wulp.)

= *A. vanus*, Walker.

‘Notes Leyden Museum,’ 6, p. 249, V. d. Wulp ; ‘Proc. Linn. Soc., Lond.,’ 4, p. 91, Walker (1860) ; ‘Mono. Culicid.,’ 1, p. 142, Theobald (1901).

This sub-species is like *A. sinensis*, but the cross-veins are further apart, and the costal spots smaller, and the palpi and proboscis rather more scaly. There is no pale patch in the wing fringe where the lower branch of the fifth vein joins the border, and the majority of specimens are much smaller than the type. There is a considerable amount of variation in regard to the leg banding and the banding in the palpi.

Length.—Female, 4 to 5 mm. ; male, 3·8 to 4·5 mm.

Habitat.—Madras ; Sambalpur, Central Provinces ; Straits Settlements.

Sub-species *Indiensis*. (Theobald.)

Fork cells much longer than in the type ; the base of the first sub-marginal being close to the first costal spot, close to the junction of sub-costal and costal. Fringe pale at the lower branch of the fifth long vein, whilst the cross-veins resemble most nearly those of *A. annularis* (type).

Habitat.—Madras.

Sub-species *nigerrimus*. (Giles.)

‘Hnd. Bk. Gnats,’ p. 161 (1900), Giles ; ‘Mono. Culicid.,’ 1, p. 145 (1901), Theobald.

Near to *annularis*, but there is a pale patch at the end of the lower branch of the fifth long vein, and the apical fringe is black.

Habitat.—Calcutta ; Naini Tal ; Lahore ; Travancore ; Dacca.

Note.—There is a slight difference in the wing scales to the other sub-species and the type, the scales being rather shorter and broader.

Anopheles barbirostris. (Van der Wulp.)

'Leyden Museum Notes,' 6, p. 48, Van der Wulp; 'Mono. Culicid.,' 1, p. 146 (1901), Theobald.

Thorax deep grey with slaty reflection, dark longitudinal lines and minute black specks, with long scattered golden hair-like scales. Palpi densely scaled, entirely black, palpi and scaled proboscis forming a very prominent black projecting mass. Abdomen nearly black. Legs dark brown, usually unbanded, but with sometimes narrow apical and basal banding. Costa black, with two small yellow spots; apical fringe black, except between the second and third veins. Scales much more robust than in *A. sinensis* and its sub-species.

Length.—5 mm.

Habitat.—Calcutta; Selangor; Old Calabar (West Africa).

Observations.—This Malay and East Indian species, which has also recently been found in India, can at once be told from the others of this group by the entirely black palpi and the much broader wing scales. Dr. Christophers sent me a note (undoubtedly concerning this species) from Gopal Chunde Chatterpee, stating that in living specimens the halteres are to be seen rhythmically moving backwards and forwards, and thus differing from sub-species *nigerrimus*, in which the halteres are not so fully developed, and do not move about.

Anopheles gigas. (Giles.)

'Ento. Mo. Mag.,' p. 196, vol. 37, 1901, Giles; 'Mono. Culicid.,' 2, p. 308, 1901, Theobald.

Thorax fawn coloured in the middle, dark brown on each side; abdomen dark brown; palpi long and thin, deep brown; legs dark brown, with basal pale banding, especially to the hind legs, bases of the legs pale, almost white.

Wings with two large black spots on the costa and black patches on the wing-field; fringe pale where the veins join the border of the wings except at the ends of the fifth vein, which have a pale patch *between* them.

Length.—6 to 7 mm.

Habitat.—Conoor, Nehilgerri Hills, India.

Observations.—The largest and handsomest Indian anopheles I have seen. The two pronounced black costal spots should at once identify it. Taken at an elevation of 6,000 feet.

Anopheles metaboles. n. sp.

Thorax brown, with frosty-grey scales in the middle, the brown forming a broad line on each side. Abdomen deep brown with golden

hairs and small scales. Legs deep brown speckled with white, joints of the fore and hind tarsi with apical white spots. Wings with the costa with four prominent black spots and two smaller basal ones, and a few small black spots on the veins; fringe dark, with pale areas.

♀. Head dark brown with frosty upright forked scales in front, dark brown ones behind and at the sides; antennæ dark brown; palpi deep brown with three white bands, the two apical ones the widest, the whole of the last joint is white; proboscis black. Thorax deep brown with narrow curved, flat, pale creamy scales, giving the thorax a frosty appearance in the middle, and a median tuft of white hair-like scales projecting over the top of the head; the sides of the mesothorax dark; scutellum paler brown, with narrow curved pale scales and dark brown border-bristles; pleuræ chestnut-brown, with patches of creamy scales.

Abdomen deep brown, with dense dull golden hairs, and scattered small narrow curved pale scales.

Legs dark brown, speckled with pale creamy spots; apices of the fore tibiae, metatarsi, and first two tarsi yellow, last two all dark brown; in the mid legs the apical spots are scarcely apparent; in the hind more distinct, but the last joint is all dark brown; unguis small, equal, and simple.

Wings (Plate 5, fig. 1) with four large black costal spots, and two smaller ones near the base; the two apical costal spots are continuous.

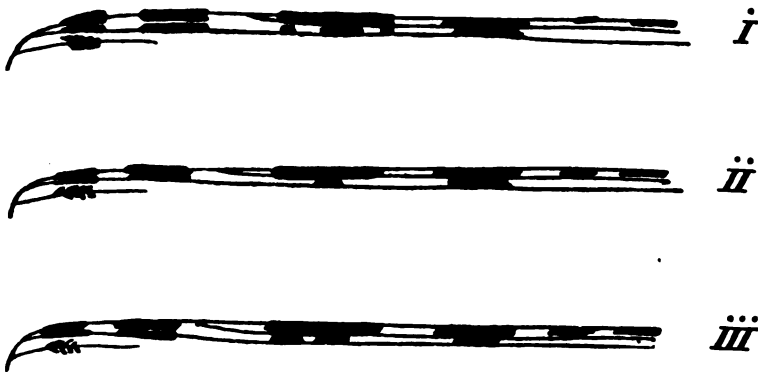


FIG. 1.—Three variations in the wing of *A. metaboles* ♀.

on to the first long vein; the third, which is the largest, has three typical spots beneath it on the first long vein (fig. 1, i); the fourth has one beneath it not quite so long; there are also black spots on the veins as follows: one on the upper branch of the first fork-cell, just below the apical costal spot, another towards the base of the fork-cell, a small one near its apex, and two small ones on the stem; two small spots at the base, and one at the apex of the third vein; two on the upper and one.

on the lower branch of the second fork-cell, and two long ones on its stem; two on the upper, one on the lower branch of the fifth long vein, and one at its base; three small ones on the sixth vein; fringe dusky, partly yellow at the apex, and with a yellow spot at the terminations of the veins; fork-cells short, base of the first submarginal slightly the nearer to the base of the wing, its stem nearly as long as the cell; stem of the second posterior longer than the cell; supernumerary cross-vein nearly its own length in front of the mid cross-vein, the posterior cross-vein longer than the mid, nearly twice its own length distant from it, stopping towards the base of the wing.

Length.—3·5 to 4·5 mm.

Habitat.—Lahore, India.

Observations.—Described from five ♀'s sent by Captain James and Drs. Christophers and Stephens. It comes near *A. maculata*, Theo. (p. 171, vol. 1, 'Mono. Culicidæ'), but can at once be told by the last hind tarsi not being white as in that speckled-legged anopheles; from the speckled-legged *A. Theobaldi*, Giles ('Ento. Mo. Mag.', p. 198, 1901), and *A. Jamesii*, Theo. ('Mono. Culicidæ', p. 134, vol. 1), also by the hind tarsi not being white. The third spot on the border of the wing is subject to some variation, as shown in fig. 1. One specimen has only two spots on the sixth long vein, not three.

Anopheles Rossii. (Giles.)

'Journ. Trop. Med.,' Oct. 1899, Giles; 'Hnd. Bk. Gnats,' p. 149, Giles; 'Mono. Culicid.,' 1, p. 154, Theobald.

Thorax pale yellowish-brown, with scattered greyish reflections, and with a darker median line and scattered pale scales and hairs. Palpi of ♀ dark scaled, white apically, and with two other pale bands; of ♂, swollen at the end, yellow, with a broad black band at the base, one in the middle, and a small broken one at the apex of the same joint, and a narrow black ring at the base of the last two apical joints. Abdomen dusky, with a dense covering of ochraceous hairs. Legs yellow, with brown scales and yellowish bands, both apical and basal to the tarsi. Wings with four black costal spots, the largest usually flatly T-shaped, but variable; fringe with yellow spots where the veins join the border of the wings, except at the sixth vein.

Length of ♀ 4 to 6 mm.; of ♂ 4·5 to 5·5 mm.

Habitat.—Calcutta; Lucknow; Madras; Quilon; Behar, Bengal; Dacca; Sambalpur; Etawah, N.W.P.; Singapore; Perak, and other Malay States.

Observations.—This species seems to be very variable in regard to the wing markings. The typical costal T-shaped spot will not distinguish the species as I previously supposed, as this spot is variable. Several specimens have an accessory spot at the end of the T-shaped one,

others Dr. Christophers sends me have the T-spot badly developed ; others have the first and second large spots united on the costa.

In any batch of *A. Rossii* caught at Calcutta, it seems from the joint letter of Captain James, I.M.S., and Drs. Stephens and Christophers, several variations from the type may be found.

It appears this common Asiatic species does not take part in the distribution of malaria.

Anopheles Indicus. (Theobald.)

= *A. culicifacies*. Giles. (♀.)

'Mono. Culicid.,' 1, p. 183, 1901.

Thorax pale ochraceous-brown, with pale golden hair-like curved scales and black bristles ; palpi dark brown, with yellowish-white apex and two other pale rings. Abdomen dark shiny-brown, densely clothed with golden-brown hairs ; venter pale ochraceous. Legs dark brown, almost black, a small yellow spot at the apex of the tibiae and femora. Wings with costal border marked with four prominent black spots and a small basal one ; wing-fringe dusky, with *three yellow patches*, namely, at the ends of the lower branch of the fourth and the two branches of the fifth.

Length.—3 to 3·5 mm. *Habitat*.—Madras.

Observations.—I have seen only a single specimen of this insect. It is very similar to Grassi's *A. superpictus*, but much smaller, and has no tarsal banding.*

Anopheles Listoni. (Giles.)

= *A. culicifacies*. ♀. (Giles.)*

'Ento. Mo. Mag.,' p. 197, 1901.

Thorax dark brown at the sides, paler in the middle ; abdomen deep brown, with scattered pale hairs. Legs dark brown, unbanded. Wings with the costa black, including the actual base, with four small distinct yellow spots, the largest light area being that quite at the base ; fringe dark, except at the apex, and indistinct paler patches opposite one or two of the longitudinal junctions. Palpi of ♀ black, with *two narrow rings and a minute white tip* ; in the ♂, black with two whitish bands. (Fresh specimens show three fringe spots.)

Length.—3·5 to 4 mm.

Habitat.—Ellichpur, Behars ; Etawah, N.W.P.

Observations.—This and my *A. Christophersi* are very closely related, but can easily be separated by the difference in the banding of the palpi.

* Having recently examined a fresh series of *culicifacies*, I am convinced my *A. Indicus* is the same. This name must therefore sink as a synonym, as Col. Giles described *Listoni* and *culicifacies* a month before my *Indicus*. Giles's ♀ *culicifacies* is the same as *Listoni*, so that both names sink under *culicifacies*, as the latter was described first.

Anopheles Christophersi. n. sp.

Thorax brown with narrow curved, long, pale golden scales, slightly darker at the sides. Abdomen dark brown with golden hairs. Legs dark brown, unbanded, and with no knee or other spots. Palpi brown to black, with two broad apical white bands and a narrow one towards the base. Wings with the costa black, with four small yellow spots, the basal one rather indistinct, veins with rather long scales, the black ones being most prominent; fringe black, with four pale spots.

♀. Head dark brown, with broad white upright forked scales in front, black ones behind, and a dense tuft of white hairs projecting forwards; palpi dark brown, almost black, densely scaled, with two broad apical bands and a narrow one on the apex of the basal third of the palpi; antennæ bright brown; proboscis deep brown with pale apex.

Thorax brown, with long, narrow, curved, hair-like, golden scales and brown bristles; scutellum cinereous-brown with a double row of border-bristles, the outer ones being large, the inner small; metanotum deep brown to black.

Abdomen almost black, with golden hairs. Legs completely dark brown with bronzy reflections in the sun; ungues equal and simple.

Wings (Plate 5, fig. 3) with the costal border black, with four small yellow spots, and the apex yellow, the basal spot very small; the yellow spots spread uniformly on to the first long vein except the third which passes well under the black costal spot; scales of the veins mostly black; a yellow patch at the base of the fork-cells and on most of the third long vein, a long patch at the base and another towards the middle of the fourth vein; one large and one small patch on the upper and one large one on the lower branch of the fifth vein, the stem mostly pale; most of the sixth dark, but there is a broad yellow line towards the base; fringe black, with yellow spots at the end of the veins, except where the sixth joins the costal border; first submarginal cell considerably longer and narrower than the second posterior cell, its base much the nearer to the base of the wing; its stem about half the length of the cell; stem of the second posterior about one and a half times the length of the cell; supernumerary cross-vein about half its own length in front of the mid cross-vein; the mid more than its own length in front of the posterior. Halteres with pale testaceous stem and black knob.

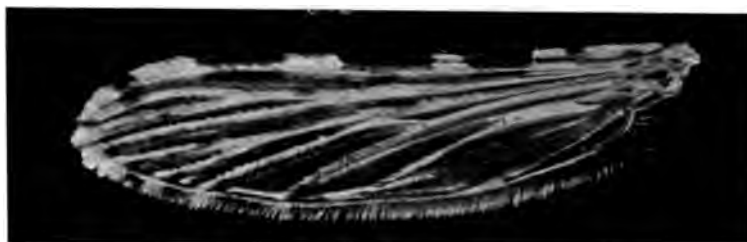
Length.—2.5 mm.

Habitat.—Duars (Christophers).

Observations.—Described from two ♀'s sent to the Royal Society.

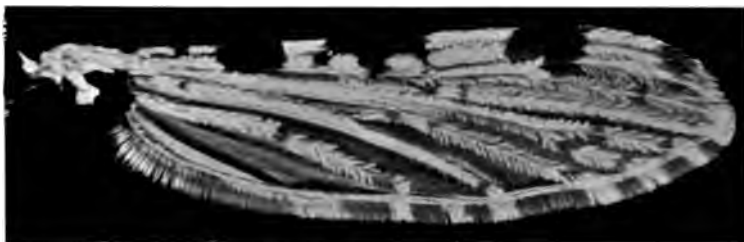
The species is very closely related to my *A. Indica*, but has more than three pale fringe spots, and the cross-veins are not the same, being more like *A. superpictus*, Grassi. The long black basal part of the costa is also very characteristic, as also is the pale third long vein.

FIG. 1.



Anopheles metaboles, n. sp. (♀).

FIG. 2.



Anopheles pulcherrimus, n. sp. (♀).

FIG. 3.



Anopheles Christophersi, n. sp. (♀).

[From negatives taken by
Mr. W. H. Hammond.]



SECRET

1

The only other unbanded legged Indian species related to it are *A. Listoni*, Giles, *A. culicifacies*, Giles, and *A. Turkhudi*, Liston; the first has the largest right costal area at the base of the wing and the forked scales of the head mostly dark in the ♀, and the ♀ palpi with two narrow rings and a minute white tip. *A. culicifacies* has only three pale fringe spots.

A. Turkhudi differs in having the apex of the palpi black in the ♀.

A. Christophersi is abundant in the malarious district of Duars, and is a prominent malaria bearer.

Anopheles Turkhudi, Liston.

A. culicifacies ♂. Giles.

'Indian Med. Gazette,' December 1901, p. 441.

"Thorax covered with white scales on a dark background, scales so arranged as to show a median and two rather less distinct, lateral, darker, longitudinal markings. Lateral aspects of the thorax covered with brown scales, often tipped with white. Abdomen dark olive-green colour, covered with many light yellow hairs. Legs dark scaled, except for a few yellowish-white scales at the distal extremities of the femora and tibiae. Proboscis dark, yellow at tip. Palpi with black scaled tips, and with three white scaled bands on each; wing with six white scaled portions to the costa, one at the apex of the wing; the first long vein has five white areas corresponding with the five outer white ones on the costa, in addition a small white interruption at the position of the marginal transverse vein; fringe with pale interruptions, except at the sixth vein.

"*Habitat*.—Ellichpur. *Time of Capture*.—March and April."

Note.—I have not seen this species. It is described by Captain Liston, I.M.S., and seems clearly distinct. Unfortunately no measurements are given in his paper. The figure given shows the tarsi banded with narrow pale bands, thus differing from the description in the text. The black-tipped palpi should easily enable the observer to identify this *Anopheles*.—(F. V. T.)*

Anopheles culicifacies. (Giles.) ♀.

A. Listoni. Giles.

A. Indica. Theobald.

'Ento. Mo. Mag.' p. 197 (1901), Giles; 'Mono. Culicid.', ii, p. 309 (1901), Theobald.

Thorax brown, darker at the sides; abdomen brown, with deep brown apical bands, slightly darker at the sides. Legs dark brown, unbanded. Wings with four almost equal sized pale spots on the costal

* Since this went to press, Capt. Liston has presented the types to the British Museum.

border and one smaller basal one: third long vein dark. Palpi of ♀ thin, brown with two narrow pale bands and a pale apex; of ♂ brown, the last two joints swollen, the penultimate and antepenultimate with apical pale bands, the apex of antepenultimate and the penultimate with golden-brown hair-tufts.

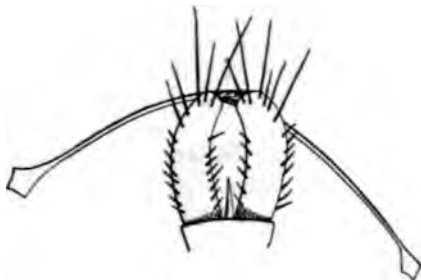


FIG. 2.—Male genitalia of *A. culicifacies*. ($\times 50$).*

Length of ♀ 3.8 mm.; of ♂ 3.5 to 4 mm.

Habitat.—Hoshangabad (C.P.), and Behars; Lahore, Punjab.

Observations.—A very distinct species. The abnormal habit of assuming the position of *Culex* when resting should also identify it when alive. The abdomen when fresh seems to have conspicuous banding. The wing-fringe in the ♀ type is unspotted, but this is due to fading; fresh specimens have three spots.

The male genitalia (fig. 2) are very peculiar.*

Anopheles Kochii. (Donitz.)

'Insecten Börse,' 5 Jr., 18, 31 January, 1901, p. 36, Donitz; 'Mono. Culicid.,' 1, p. 174 (1901), Theobald.

Thorax fawn coloured, with two large eye-like black spots; the front with longish white, the rest with creamy curved scales. Abdomen ashy-brown with numerous scales and dull golden hairs and lateral dark tufts of scales as in *A. pulcherrimus*, Theobald. Legs banded and marked with dark brown and yellow, some of the tarsi basally brown banded. Costa creamy-yellow with four black spots and numerous small ones over the wing field.

Length.—3 mm.

Habitat.—Malay Peninsula, East Indies (Sumatra and Java); Calcutta.

Observations.—Very like *A. pulcherrimus*, and closely related to it, being densely scaly, and with similar lateral apical tufts to the abdominal segments, but easily separated by the two thoracic ocelli and the absence of white hind tarsi.

* The ♀ type of *culicifacies* is the same as Giles's ♀ *Listoni*; as *culicifacies* was described first, that name stands. The ♂ type of *culicifacies* is a ♂ *A. Turkhadi*, *Liston*.

Anopheles Lindesayii. (Giles.)

'Hnd. Bk. of Gnats,' p. 166 (1900), Giles; 'Mono. Culicid.,' 1, p. 203 (1901), Theobald.

Thorax ashy-grey in the middle, a dark brown stripe on each side, with three dusky median lines and golden hair-like scales. Abdomen black, with numerous pale and dark hairs. Costa black, with one creamy patch near the apex; veins mostly black scaled; legs black; posterior femora with a broad white band in the middle.

Length.—4.5 mm.

Habitat.—Bakloh, Punjaub; Naini Tal; Kurseong (Darjeeling) Mussoorie.

Observations.—A very distinct species, at once told by the broad white band on the posterior femora.

Genus *Toxorhynchites*. (Theobald.)

'Mono. Culicidæ,' 1, p. 244 (1901).

This genus resembles *Megarhinus*, but the ♀ palpi are short and thick, and three-jointed. Several species described as *Megarhinus* will have to come in this genus, including the Indian species.

Toxorhynchites immisericors. (Walker.)

Megarhinus immisericors. (Walker.)

Culex regius. (Thwaites.)

Megarhinus Gilesii. (Theobald, 1901.)

'Proc. Linn. Soc. Lond.,' iv, p. 91 (1860), and vii, p. 202, Walker;

'Mono. Culicid.,' 1, p. 225 (1901), Theobald.

This large mosquito, called the elephant mosquito, can at once be told from other Indian *Culicidæ* by the caudal tuft. Walker described it as a *Megarhinus*. Not having seen the ♀ (as I imagined) when my recent monograph of 'Culicidæ' went to press, I included the ♂ in that genus. The ♀'s have short palpi, and thus the species comes in my genus *Toxorhynchites*. There is considerable variation in the leg banding. The ♀ specimens I described as *M. Gilesii* I now find are only Walker's species *immisericors*.

Habitat.—Travancore; Trincomalie, Hot Wells; Ceylon; Celebes; Mysol; North Ceram; Weigiou; Sikkim; Burma.

Genus *Mucidus*. (Theobald.)

'Mono. Culicid.,' 1, p. 268 (1901).

This genus can at once be identified by the curious twisted scales, giving the insects a mouldy appearance.

Mucidus scataphagoides. (Theobald.)

'Mono. Culicid.,' 1, p. 277 (1901), Theobald.

Thorax densely covered with loose scales, giving it a grey, mouldy hue, with a yellowish-brown patch on each side; having the appearance of the head of an insect. Abdomen yellowish-brown, with a white median tuft of scales on the first four segments, remainder mostly white scaled. Wings with fringe with alternate pale and dusky patches; scales on the veins dense, forming dusky and ochraceous patches. Legs ochraceous banded with ochraceous-yellow and brown parti-coloured scales and white scales.

Length.—6.5 to 7 mm.

Habitat.—Moradabad (N.W.P.) and Burma.

Observations.—Easily told from all other Indian mosquitoes by its large size and curious twisted scales.

Genus *Stegomyia.* (Theobald.)

'Mono. Culicid.,' 1, p. 283 (1901).

Thick set culices with flat scales and upright forked-scales over the head only and flat scales on the scutellum.

Five species of this genus occur in India, which may be tabulated as follows :—

A. Tarsi basally banded.

a. Proboscis banded distinctly in ♂, less so in the ♀.

Wings with black scales and scattered white ones.

pipersulata. (Giles.)

aa. Proboscis unbanded.

Thorax with two median parallel pale lines and lateral curved pale lines.

fasciata. (Fab.)

Thorax with a single median silvery line and lateral curved lines.

scutellaris. (Walker.)

B. Tarsi apically and basally banded.

Thorax with narrow median silvery line and lateral curved lines and two short yellow ones in front.

pseudotæniata. (Giles.)

Thorax sooty, with a round anterior median and four lateral snowy spots at corners of the mesonotum.

gubernatoris. (Giles.)

Stegomyia pipersulata. (Giles.)

'Hand Bk. Mosq.,' 2nd ed. (1901), Giles.

'Mono. Culicid.,' ii, p. 316 (1901), Theobald.

Clearly told by the white and black wing scales from other Indian

species. I have only seen the types presented to the Museum. (Col. Giles.)

Habitat.—Jhansi and Gonda, N.W.P.

Stegomyia fasciata. (Fab.)

'Syst. Antl.,' 36, 13 (1805), Fab.; 'Mono. Culicid.,' 1, p. 289 (1901).

This common household species has been recorded from Quilon, Calcutta, Madras.

It does not seem to be so abundant in India as in other parts of the world. This is the species which is connected with yellow fever. It is known under at least fifteen different names, the commonest being *Culex teniatus*, Wiedemann.

Stegomyia scutellaris. (Walker.)

C. albopictus. (Skuse.)

C. variegatus. (Doleschall.)

'Proc. Linn. Soc. Lond.,' iii, p. 77 (1859), Walker; 'Mono. Culicid.,' 1, p. 289, Theobald.

An abundant Asiatic species, occurring in India at Madras, Naini Tal, Sambalpur (C.P.); Calcutta, &c. It is the same as Skuse's *Culex albopictus*, recorded from Calcutta, where it is a great nuisance.

Stegomyia pseudoteniata. (Giles.)

'The Entomologist,' p. 192 (1901), Giles; 'Mono. Culicid.,' 1, p. 312 (1901), Theobald.

I have only seen a single specimen of this marked *Stegomyia*, and have no further notes to add.

It occurred at Naini Tal (7,000 feet), Bakloh, Punjab (6,000–8,000 feet).

Stegomyia gubernatoris. (Giles.)

'The Entomologist,' p. 194, July (1901), Giles; 'Mono. Culicid.,' 1, p. 314 (1901), Theobald.

A single specimen only has so far been taken by Colonel Giles in Government House, Allahabad. The marked thoracic ornamentation mentioned in the table makes its identification an easy matter. Evidently a rare species.

Genus *Armigeres*. (Theobald.)

'Mono. Culicid.,' 1, p. 322 (1901).

Separated from *Stegomyia* by the long, thin, hairless ♂ palpi and peculiar larva.

Armigeres obturbans. (Walker.)

Culex obturbans. (Walker.)

Culex ventralis. (Walker.)

'Proc. Linn. Soc. Lond.,' iv, p. 91 (1860), et v, p. 144, Walker ;

'Mono. Culicid.,' 1, p. 323 (1901), Theobald.

Thorax black, covered with bronzy scales in the middle of the mesothorax and with creamy-white ones all round, forming a broad, pale band. Abdomen black, with large white triangular lateral spots, last segment white. Legs black, bases and venter of the femora pale, almost white.

Length.—6·5 to 7·5 mm.

Habitat.—Madras ; Naini Tal ; Travancore ; Morzufferpur, Behar, Bengal ; Malay Peninsula ; Hong Kong ; Formosa ; Foo Chow ; East Indies.

Observations.—This is one of the most abundant Asiatic species.

Lieut.-Colonel Giles has sent me specimens from India under different specific names, but only one seems at all distinct. The species is certainly subject to considerable variation in size.

If the species of this genus are to be separated I am sure they can only be done so by an examination of the male unguis and genitalia. For the present I retain them all but one in this species. The typical ♂ unguis are as follows—the fore unequal, both uniserrated, the mid smaller than the fore, not so unequal and uniserrated ; the hind small, equal and simple.

Armigeres panalectros. (Giles MS.)

'Mono. Culicidæ,' 2, p. 317 (1901).

This species is much smaller than *A. ventralis*, and has the thorax elaborately adorned ; mouse coloured adorned with lighter lyre-shaped marks, and all the unguis of the male are toothed.

Habitat.—India.

Genus *Culex*. Linnæus.

'Linn. Syst. Nat.' (1735), Linnæus ; 'Mono. Culicid.,' 1, p. 326 (1901), Theobald.

The type may be taken as *C. fatigans*, Wied., or *C. pipiens*, L., head scales and thoracic scales being mostly narrow curved ones.

Sixteen species of *Culex* are so far known in India ; the following table will help identify them :—

A. Wings spotted.

α. Wings spotted along the costa. Proboscis banded.

mimeticus. (Noe.)

- aa. Wings faintly spotted on wing field. Thorax ornamented with lines of white scales; legs spotted; wing spots three.
spathipalpis. (Rondani.)
- AA. Wings unspotted.
- β. Legs banded.
- γ. Proboscis banded with one pale band.
- δ. Tarsi basally banded.
- ε. Abdomen with lateral white spots and basal bands; ♀ fore ungues equal and simple. *microannulatus*. (Theobald.)
- εε. Abdomen basally banded, but with no lateral spots.
Mid and hind legs with a broad tibio-metatarsal band.
impellens. (Walker.)
- No broad tibio-metatarsal band, last abdominal segments with apical and basal banding. *Vishnui*. (Theobald.)
- γγ. Proboscis with three pale bands. Tarsi basally banded.
tritæniorhynchus. (Giles.)
- γγγ. Proboscis unbanded.
- δ. Legs basally banded.
- ε. Thorax not ornamented.
Abdomen with basal and some apical bands; fore and mid ungues of ♀ uniserrated. *cantans*. (Meigen.)
- δδ. Legs basally and apically banded.
Thorax adorned with frosty-white scales on anterior two-thirds. *gelidus*. (Theobald.)
- δδδ. Legs unbanded.
- ε. Legs with a white line on femora and tibiæ.
tipuliformis. (Theobald.)
- εε. Legs uniformly coloured.
- ζ. Abdomen with lateral white spots and basal patches.
Thorax golden scaled, with lateral lines of brighter golden scales on posterior half; abdomen with curved lateral silvery spots. *pulcriventer*. (Giles.)
- ζζ. Abdomen with basal pale bands.
Thorax black, with ferruginous scales, pale at the sides.
nigripes. (Zett.)
- ζζζ. Abdomen apically banded
First few segments with yellow apical bands, remainder all ochraceous. *concolor*. (Desvoidy.)

Pipiens Group.

- α. Abdomen basally banded.
- β. Thorax unadorned.
Abdominal bands swollen out in middle, apical segments white. *viridiventer*. (Giles.)
Abdominal bands uniform. *quasipipiens*. (Theobald.)

ββ. Thorax adorned.

Thorax with two or three dusky, longitudinal lines; basal abdominal bands curved, white or creamy.

fatigans. (Wiedemann.)

Abdominal bands swollen into a median, triangular patch.

angulatus. (Theobald.)

Culex mimeticus. (Noe.)

'Boll. d. Soc. Ent. Ital.,' 31, p. 240 (1897), Noe;

'Mono. Culicid.,' 1, p. 329 (1901), Theobald.

Thorax brown, with dark stripes and with golden curved scales. Abdomen brown, with basal white bands; venter white. Legs brown, metatarsi and tarsi with basal white bands and traces of apical ones. Proboscis banded. Wings spotted with brown and yellow along the costa, three yellow spots.

Length.—5 to 6 mm.

Habitat.—Punjaub; Perak; Italy.

Observations.—Apparently a common species in parts of Northern India and Malay Peninsula. Easily told by its wings, resembling those of an *Anopheles*. There seems to be considerable variation in size in the specimens I have recently seen.

Culex spathipalpis. (Rondani.)

'Dipt. Ital. Prodro.,' vol. 1 (1886), Rondani; 'Mono. Culicid.,' 1, p. 339 (1901), Theobald.

Thorax umber-brown, with a median line of white scales, a bent white lateral line, and another more indistinct on each side below. Abdomen yellow scaled, with scattered black ones which form patches, basally banded white, the bands expanding backwards in the middle of the second and third segments and produced laterally. Legs with lines of white spots, often almost forming stripes; metatarsi and some of the tarsi basally banded white. Wings with three small more or less distinct patches of scales.

Length of ♀ 7 to 8 mm.; of ♂ 7 to 8 mm.

Habitat.—India (Naini Tal); Italy; Gibraltar; Palestine.

Observations.—A very marked species occurring in Europe and India. The spotted legs and wings and its large size will at once separate it.

Culex microannulatus. (Theobald.)

'Mono. Culicid.,' 1, p. 353 (1901), Theobald.

Thorax brown, with curved golden-brown scales and three rows of dark bristles; abdomen dark brown with basal greyish-white bands.

Proboscis with a broad yellowish-white band in the middle. Legs brown, with small yellow basal rings and a small white band at the apex of the femora. Head brown, covered with creamy-brown curved scales and a pale border round the eyes and upright brown forked scales.

Fore unguis of ♂ unequal, the larger uniserrated, the smaller simple.

Length of ♀ 5 mm.; of ♂ 5 mm.

Habitat.—Quilon, Travancore; Madras; Calcutta, and North-West Provinces.

Observations.—Very like the next species, only darker, rather larger, and the head all pale instead of with two black patches as in *C. Vishnui*. The ♂ unguis also differ.

Culex Vishnui. (Theobald.)

'Mono. Culicid.,' 1, p. 355 (1901).

Thorax brown, with fawn coloured scales, two more or less distinct median lines and a thin central one formed by a row of bristles. Abdomen deep brown, basally banded with pale yellow, some of the apical segments with a narrow row of apical yellow scales as well. Legs brown, tarsi darker, narrowly basally banded with yellow; unguis of ♀ equal and simple. Head with dark lateral patches. Male fore unguis, unequal, both uniserrated.

Length of ♀ 3 to 4.5 mm.; of ♂ 3 to 4 mm.

Habitat.—Quilon; Sambalpur; Punjaub; Madras; Dacca; Ceylon.

Observations.—Resembling the former, but easily told by the paler hue and head ornamentation, as well as the ♂ unguis. Evidently a common southern form. Not so far recorded from Northern India.

Culex impellens. (Walker.)

'Proc. Linn. Soc. Lond.,' iv, p. 91, Walker; 'Mono. Culicid.,' 1, p. 362 (1901), Theobald.

Thorax dark brown, covered with pale golden curved scales. Abdomen covered with dusky-brown scales, with basal white bands. Legs brown, the mid and hind legs with a pale band involving both sides of the tibio-metatarsal joint; tarsal banding basal. Bases of fork-cells nearly level.

Length.—♀ 4.5 mm.

Habitat.—N.W. Provinces; Calcutta; Perak.

Observations.—Closely related to *C. microannulatus*, but easily told by the tibio-metatarsal banding.

Culex tritaeniorhynchus. (Giles.)

'The Entomologist,' p. 192 (1901), Giles; 'Mono. Culicid.,' 1,
p. 364 (1901), Theobald.

Thorax fuscous with golden-brown tomentum, unadorned. Abdomen fuscous with narrow yellowish-white basal bands, broader in the middle than laterally. Legs minutely basally banded; proboscis with three ochraceous bands.

Length of ♂ 2·4 mm.; of ♀ 3 mm.

Habitat.—India.

Observations.—I have not been able to find this species amongst those sent me by Colonel Giles, nor have I seen any triple-banded proboscis species. Several specimens of *C. Vishnui* have appeared so through denudation however.

Culex cantans. (Meigen.)

C. stimulans. (Walker.)

C. maculatus. (Meigen.)

C. fumipennis. (Stephens.)

'Syst. Besch.,' 1, 6, 6, Meigen (1818); 'Mono. Culicid.,' 1,
p. 401 (1901), Theobald.

Thorax dark brown, covered with narrow, reddish, and golden-brown curved scales, paler and broader just before the scutellum and at the sides of the mesonotum. Abdomen dark brown, with basal pale bands which often spread out laterally on some of the segments; the last few segments have traces of apical banding also, the apical segments often entirely pale scaled. Legs dark brown, mottled with paler scales, metatarsi and tarsi with pale broad basal bands; fore and mid unguis of ♀ thick, equal, uniserrated; hind simple.

Length of ♀ 6 to 7 mm.; of ♂ 7 mm.

Habitat.—Europe, N. America, and India.

Observations.—A very distinct, large species common in Europe. The mid cross-vein and supernumerary in one line. Colonel Giles has given an undoubted specimen of this species to the British Museum. It was taken by Dr. Price at Conoor in the Nehilgerry Hills at an elevation of 6,000 feet.

Culex gelidus. (Theobald.)

'Mono. Culicid.,' ii, p. 20 (1901), Theobald.

Thorax dusky yellowish-brown to deep brown, with the anterior two-thirds covered with frosty-white curved scales, the hind portion with almost black scales and long black bristles. Abdomen brown with basal creamy-white bands and pale lateral spots. Unguis of the ♀ equal and simple.

Length of ♀ 4.5 mm.

Habitat.—Quilon, Travancore ; Dacca ; Ceylon ; Perak ; Selangor.

Observations.—A very distinct species with a wide distribution in India. The frosty anterior two-thirds of the thorax will identify it at once.

A distinct variety, *cuneatus*, occurs in Southern India at Quilon, in which the abdominal bands have a triangular median projection.

Culex tipuliformis. (Theobald.)

'Mono. Culicid.,' ii, p. 325 (1901), Theobald.

Thorax brown, with narrow, golden-brown curved scales, paler behind ; pleuræ white scaled. Abdomen dark brown, with basal white median semicircular patches on segments two to five, basal bands spreading out laterally on the apical segments ; legs unbanded, paler brown, long and thin ; femora and tibiæ with a distinct line of white scales running down them. Proboscis with apparently two pale bands.

Length.—5.8 mm. (♀).

Habitat.—Bakloh (N.W.P.).

Observations.—A very distinct large species, with peculiar leg ornamentation. I am not certain of the proboscis banding, having seen only a single specimen.

Culex pulcriventer. (Giles.)

'The Entomologist,' p. 194 (1901), Giles ; 'Mono. Culicid.,' ii, p. 48 (1901), Theobald.

Thorax black, with a patch of golden scales in front, a line of golden scales on each side, along posterior half of mesonotum, curved downwards in front, and a row of golden scales on each side of the bare space in front of the scutellum. Abdomen black, with basal silvery white curved lateral patches ; venter golden-yellow, with basal white patches, and a narrow black border to the segments. Legs black, basal parts creamy white.

Length of ♀ 5.5 to 6.5 mm. ; of ♂ 5 to 5.5 mm.

Habitat.—Naini Tal, India.

Observations.—Easily identified by the peculiar thoracic ornamentation and the basally white femora.

Culex nigripes. (Zetterstedt.)

C. impiger. (Walker.)

C. implacabilis. (Walker.)

'Ins. Lapp.' (1838-40), Zetterstedt ; 'Mono. Culicid.,' ii, p. 93 (1901), Theobald.

Thorax black, with pale ferruginous scales, paler at the sides.

Abdomen black, with basal white bands, which spread out laterally on the last few segments. Legs black; femora pale beneath; fore and mid ungues of ♀ equal, unserrated.

Length of ♀ and ♂ 4·5 to 6 mm.

Habitat.—N. Europe; N. America and India (Kashmir); Greenland and Arctic Circle generally.

Observations.—Some damaged specimens have been received by the British Museum (Nat. Hist.) from Dr. Neve from the Deosai Plateau, between Kashmir and Shardo, between 13,000 and 13,500 feet. They are undoubtedly this species which seems to spread out round the north polar region to about 35° latitude.

Culex concolor. (Robineau Desvoidy.)

‘Mém. Soc. Hist. Nat. de Paris,’ 4, 405, Rob. Desv.; ‘Mono. Culicid.,’ ii, p. 107 (1901), Theobald.

Thorax brown, with tawny and fawn-coloured curved scales, with patches of paler scales in the middle line in front, a central patch, two lateral ones, and other pale scales in front of the scutellum. Abdomen with brown scales, the first few segments with apical dull creamy borders; the fifth with a dark basal band, most of it and all the remaining segments densely ochraceous yellow; wings with a yellowish tinge.

Length of ♀ 6 to 8 mm.; of ♂ 7 mm.

Habitat.—Asia generally.

Observations.—A common and widely distributed Asiatic species occurring in China, all over the Malay Peninsula, India, and Ceylon. Easily told by its general ochraceous fawn-coloured hue.

Culex viridirenter. (Giles.) •

‘Journal Bombay Nat. Hist. Soc.,’ p. 609, No. 4, vol. 13 (1901), Giles; ‘Mono. Culicid.,’ ii, p. 128 (1901), Theobald.

Thorax dark brown, with short, narrow curved deep golden-brown to bronzy scales, paler in front of the scutellum. Abdomen dusky black, with basal white bands on the second and third segments, not always complete; in the fourth to sixth the bands are very distinct and swell out in the middle; apical segment white. Legs black; coxæ and bases of femora greyish-white; knee spots distinct.

Length of ♀ 5·5 to 6 mm.; of ♂ 4 to 5 mm.

Habitat.—Naini Tal, India.

Observations.—A purely sylvan species, easily told from its nearest ally *C. fatigans* by the abdominal ornamentation.

Culex fatigans. (Wiedemann.)

Culex aestuans. (Wied.)

Culex pungens. (Wied.) ?

Heteronychia dolosa. (Arribalzaga.)

'Auss. Zweif. Ins.,' p. 10, Wiedemann, &c. ; 'Mono. Culicid.,' ii, p. 151 (1901), Theobald.

Thorax brown, with two distinct dark lines on the denuded surface, covered with pale golden curved scales, with two more or less bare parallel dark lines and three rows of dark bristles. Abdomen dark brown to black, with basal white or pale creamy curved bands and white lateral spots. Legs brown ; knee, and sometimes the apex of tibia, with a pale spot.

Length of ♀ 4·5 to 5·5 mm. ; of ♂ 4 to 5 mm.

Habitat.—Generally over India and most tropical and sub-tropical countries and even in warmer temperate countries. Subject to much variation, but easily told from its nearest ally, *C. pipiens*, L., by the relative greater length of the stem of the first sub-marginal cell.

The species will probably have to be divided up into a number of sub-species. Some of these I have mentioned in the 'Monograph of Culicidæ' (pp. 156–189). It is the chief *Filaria* carrier.

It is one of the commonest household pests in warm climates.

Culex quasipipiens. (Theobald.)

'Mono. Culicid.,' ii, p. 136 (1901), Theobald.

Thorax brown, with narrow golden curved scales ; abdomen brown, with white or pale creamy curved basal bands, last segment often entirely pale scaled. Legs unbanded brown ; knee spot pale. Wings like *C. pipiens*, but the stem of first sub-marginal cell longer, and the relative positions of the sub-costal and costal junction with the base of the first sub-marginal cell different, and the thoracic scales broader.

Length of ♀ 5 mm.

Habitat.—Sambalpur (C.P.), India.

Observations.—Very like *C. pipiens*, but differs in wing venation and in scale structure. *C. pipiens*, L., has so far not been observed in India.

Culex angulatus. (Theobald.)

'Mono. Culicid.,' ii, p. 324 (1901), Theobald.

Thorax deep brown, with narrow curved dull golden-brown scales ; pleuræ pale. Abdomen dark brown, with yellow basal bands with a triangular expansion in the middle of each. Legs long and thin, brown.

Length of ♀ 4 to 4·5 mm.

Habitat.—Naini Tal.

Observations.—Very like *C. fatigans*, from which it can be told by the relatively longer legs, smaller thoracic scales, and abdominal ornamentation.

Genus *Panoplites*. (Theobald.)

‘Mono. Culicid.’ ii, p. 173 (1901), Theobald.

Three species of this genus occur in India. They may easily be identified from other culices by the broad asymmetrical wing scales. The table below will be sufficient to identify them:—

A. Thorax unspotted. *uniformis*. (Theobald.)

B. Thorax spotted.

Yellowish-brown; wings with border-scales broad and asymmetrical. *annulifera*. (Theobald.)

Blackish-brown; wings with border-scales narrow. *annulipes*. (Walker.)

1. *Panoplites uniformis*. (Theobald.)

Panoplites Africanus. (Theobald.)

‘Mono. Culicid.’ ii, p. 180 (1901), Theobald.

An abundant species in South India, and also occurs in the Malay Peninsula.

I feel certain now that the *Panoplites* from Africa that I have described as *P. Africanus* (‘Mono. Culicid.’ ii, p. 187) is only this species. Undoubted *P. Africanus* have been sent me by Mr. Green from Ceylon. The African form is very variable, and I now find the same of the Indian.

2. *Panoplites annulifera*. (Theobald.)

‘Mono. Culicid.’ ii, p. 183 (1901), Theobald.

Occurs all over India, Malay Peninsula, and East Indies. It has also been recently sent me from Manila. Easily told from the following by its much paler hue, smaller size, and more banded and spotted appearance of the legs.

3. *Panoplites annulipes*. (Walker.)

C. dives. (Schiner.)

C. nero. (Doleschall) ?

‘Proc. Linn. Soc. Lond.’ 1, p. 5 (1857), Walker; ‘Mono. Culicid.’ ii, p. 185 (1901), Theobald.

Much the same distribution as the above, but most abundant in the

Malay Peninsula, and does not seem to occur in South India. Much darker and larger than the former.

Genus *Tœniorhynchus*. (Arribalzaga.)

'Dipt. Argentina,' p. 47 (1899), Arribalzaga; 'Mono. Culicid.,' ii, p. 190 (1901), Theobald.

A single species of this genus occurs in India. The chief character of the genus as modified by me is the presence of dense thick elongated wing scales either ending obliquely, or bluntly pointed.

Tœniorhynchus ager. (Giles.)

'Entomologist,' p. 196, July (1901), Giles; 'Mono. Culicid.,' ii, p. 199 (1901), Theobald.

Thorax deep chestnut-brown, with scattered golden scales, almost white over the roots of the wings. Abdomen black, with apical creamy bands. Legs ochraceous, with black and white scales, giving them a mottled appearance; tarsi basally pale banded on the fore legs, apically and basally on the mid and hind legs.

Length of ♂ 4.5 mm.; of ♀ 5 mm.

Habitat.—Madras and N.W. India.

Observations.—This banded proboscis species can at once be told from other Indian Culicidæ by the dense wing scales.

Genus *Aedeomyia*. (Theobald.)

'Mono. Culicid.,' ii, p. 218 (1901), Theobald.

Palpi short in both sexes. Separated from *Aedes* on account of the broad asymmetrical wing scales like *Panoplites*. A single species only occurs in this genus.

Aedeomyia squammipenna. (Arribalzaga.)

Aedes squammipennis. (Arribalzaga.)

'El Nat. Arg.,' 1, 151, 3 (1878), and 'Dipt. Arg.,' p. 62 (1891), Arribalzaga; 'Mono. Culicid.,' ii, p. 219 (1901), Theobald.

Thorax brown, with scattered creamy-yellow scales in the middle, and with white scales at the sides and behind; scutellum ochraceous, with black scales at the sides and creamy ones in the middle; abdomen brown, with two patches of creamy scales apically and two patches of white scales basally, apical segments yellow scaled. Legs banded, covered with creamy, purple, and white scales, apices of mid femora with a dense tuft of dark scales, and sometimes the hind legs are tufted.

Length of ♀ 4 to 4.5 mm. ; of ♂ 3.5 mm.

Habitat.—South America and India generally.

Observations.—I can see no difference between the Indian and South American specimens. The insect is very marked, and can at once be told by its densely-scaled dark mottled wings and leg tufts.

Genus *Uranotenia*. (Arribalzaga.)

'Dipt. Argentina,' p. 63 (1891), Arribalzaga ; 'Mono. Culicid.,' ii, p. 241 (1901), Theobald.

Told from other *Aedes* by the presence of flat scales on the mesothorax and scutellum, swollen mid femora, and by the very small fork-cells. A single species represented by two ♂'s has been found in India.

Uranotenia minima. (Theobald.)

'Mono. Culicid.,' ii, p. 262 (1901).

Thorax deep brown (denuded), with long black bristles ; metanotum with a pale median line. Abdomen dark brown, with narrow basal bands of pale yellowish-brown ; pleuræ and coxæ very pallid.

Length.—1.8 to 2 mm. (♂).

Habitat.—Quilon, South India.

Observations.—I have only seen two ♂'s of this species, both denuded of scales. They are, however, I feel certain, *Uranotenia*, but the fork-cells are a little longer than usual in this genus.

"The Application of the Kinetic Theory of Gases to the Electric, Magnetic, and Optical Properties of Diatomic Gases." By GEORGE W. WALKER, B.A. Communicated by Professor RUCKER, Sec. R.S. Received January 23,—Read February 14, 1901.

(Extended Abstract.—Received January 6, 1902.)

In this paper I have endeavoured to examine the most important magnetic, electric, and optical properties of a gas, supposed to consist primarily of two-atom molecules. A two-atom molecule, in which the atoms are of unequal mass and carry equal and opposite electrical charges, was selected not as an actual representation of the molecule, but as typical of the characteristic features. I have adopted Boltzmann's distribution law, and, as far as I can judge, have not applied it beyond the limits to which it appears to be justified experimentally.

We have first to observe that the molecules may be divided into three classes, which I have called A, B, and C.

In A the two atoms revolve in continuous contact under their mutual electric attraction; in B and C they are separated by the centrifugal tendency. In B the two atoms are describing elliptic orbits about their common centre of inertia, while in C they describe hyperbolic or parabolic orbits. In the set C the atoms may thus be regarded as practically free. We may also contemplate the possibility of multiple molecules.

The individuals of the three fundamental sets may change from time to time; but we may suppose that a permanent average distribution will finally obtain.

The proportions of molecules in the three classes must be determined by the consideration that the velocities satisfy the conditions appropriate to the specified class. There is no discontinuity in the case of B and C, and the limits of integration for these have been determined. I have not obtained a satisfactory estimate of the proportion of A to B, as a sort of discontinuity occurs. Inasmuch as the electrical energy of the two atoms when close together is very great compared with the mean kinetic energy at ordinary temperatures, the molecules are mainly of the class A.

It has thus been established that a small proportion of molecules are always dissociated, a point which has recently been established experimentally. I also find that although on the whole the numbers of B and C together diminish as the pressure decreases, yet the proportion of C to B increases as the pressure decreases. This would account for the increased ease with which electrical discharge takes place through a gas under reduced pressure.

Passing next to the magnetic properties it is shown from a former paper* that diamagnetic effects are produced by the free atoms on establishing a magnetic field, and that the effect disappears very soon. It is also shown that the molecules contribute positive magnetic susceptibility; and the formula obtained, which is complicated, agrees well with Quincke's experiments on the subject.

Turning to the dielectric constant, it is found that

$$K = 1 + k p / \theta^2,$$

where p is the pressure, θ the absolute temperature, and k is a constant depending on the gas. This differs essentially from other theories which have been proposed, as regards the temperature variation, the usual result being

$$K = 1 + k p / \theta.$$

We may see without analysis how this arises. The electrical field is capable of affecting the rotational energy of the molecule, and thus the

* "On the Phillips Phenomenon," 'Electrician,' August, 1899.

control depends on the temperature; whereas in all former theories the control is supposed to be independent of temperature.

The calculation of K applies to two-atom molecules, but it is clear that a similar result will hold for a more complicated molecule, provided that the energy of those co-ordinates, which are affected by an electrical field, can be affected by the communication of heat.

I have next considered the optical effects. The free atoms are first considered, and the method used is due to Lord Rayleigh, viz., calculating the effect of the waves on the particles, and then the modification of the waves, which these disturbed particles produce.

The motion of a free atom under the influence of plane waves can be completely determined, and the results are as follows: Each atom moves in a complicated manner, but as a first approximation the motion may be regarded as made up of a linear motion and a periodic motion. For any individual atom this periodic motion is not parallel to the plane of the waves, nor is it strictly of the same period as the incident waves; but on integration for the whole set, the component normal to the plane of the waves vanishes, and only the component in the plane of the waves remains. The free atoms accelerate the velocity of propagation by a term proportional to the square of the wave-length, and hence the refractive index is diminished by the same term. This is identical with the term introduced by Ketteler in Cauchy's formula to explain the observed facts in cases of abnormal not anomalous dispersion. The atoms scatter the light, and consequently the intensity of the transmitted light is diminished, but the effect is independent of the wave-length.

With respect to the molecules, it is found that a new distribution law obtains, the change being of such a nature that there is a periodic orientation of the axes of the molecules and a periodic surging of the centres of inertia of the molecules in the plane of the waves. Both effects contribute to the refractive index, but the former is of far greater importance than the second. The final result, which involves a function of considerable complexity, is shown to be capable of explaining ordinary or anomalous dispersion.

The formulæ for K and μ , which agree in giving $K = \mu_x^2$, are considered in their bearing on the supposed additive law. They would not give a strictly additive law, but the divergence would not be great if the properties of the constituents were not widely different. They agree with an additive law as well as experiment warrants.

The coefficient of rotation of the plane of polarisation in a magnetic field is obtained, and is found to vanish if the masses of the positive and negative atoms are equal. Combined with the formula for refractive index an estimate of $\bar{\omega}$ for oxygen is obtained, $\bar{\omega}$ being called the angular velocity of rotation of mean square, from analogy with the ordinary velocity of mean square.

I may now quote from the paper my conclusions : * “ It is interesting to note that $\bar{\omega}$ [here] plays the part of a control in refraction, and that the form of the dispersion curve is really due to the distribution law and not to radiation. The value of $\omega/2\pi$ obtained must be about three times the frequency for the Na lines and hence ω corresponds to a frequency far beyond the violet. I see no grave objection to this result, as the same result would arise in any theory in which a single term is supposed to represent the facts. The controlling period must be less than the imposed period to explain ordinary dispersion in the spectral range.

“ The high value of $\bar{\omega}$ precludes the possibility of explaining spectral lines by the molecules of this theory. I am inclined to think that spectral lines are in some way connected with dissociation, but not necessarily of simple molecules.

“ I return to a suggestion which I made earlier, that these are due to multiple molecules. We cannot avoid the conclusion that there are such in a gas. They need not, and indeed do not, have a permanent existence. It is only necessary that there should be a small proportion on average. The frequencies of these small planetary systems are probably less than the frequency of the molecule *par excellence*. Further it is probable that their frequency will not be greatly affected by pressure, but the number would probably increase with pressure. This would agree with Professor Sir Norman Lockyer’s observations that the period of the lines does not alter much with pressure, but the intensity does increase considerably with increase of pressure.”

Earlier in the paper I point out the importance of multiple molecules in connection with the theory of the ratio of specific heats. “ The ratio of specific heats for chlorine indicates more nearly six degrees of freedom, and one must either suppose that there is a considerable proportion of the set B present, or else a proportion of multiple molecules. The first supposition could hardly be supported quantitatively on electrical grounds ; while the second is more probable in view of the comparative proximity of chlorine, under ordinary conditions, to its critical state.”

Note.—In the Thesis presented to the University of Göttingen, for his doctorate, Herr Karl Baedeker has measured the effect of temperature on the dielectric constant for several gases. The thesis was published in pamphlet form in August, 1900, and in the ‘ Zeitschrift Phys. Chem.,’ vol. 36, pp. 305–335. The bearing of his results on the formula above given is analysed in the following table, θ being taken as temperature centigrade *plus* 273°·3.

* These views were first expressed in a Fellowship Dissertation presented to Trinity College, Cambridge, in August, 1900.

Sulphur Dioxide Gas.

Temperature C.	Baedecker's value of K (observed).	Calculated by formula $K = 1 + k p/\theta^2$.	Calculated by formula $K = 1 + k p.\theta$.
0	..	1·00999	1·00962
10·3	1·00928	(1·00928) basis	(1·00928) basis
16·4	1·00893	1·00889	1·00908
21·6	1·00869	1·00859	1·00892
37·5	1·00794	1·00772	1·00846
42·2	1·00765	1·00749	1·00834
49·4	1·00734	1·00716	1·00815
64·1	1·00669	1·00655	1·00780
76·1	1·00634	1·00611	1·00753
108·3	1·00551	1·00525	1·00698

Ammonia Gas.

0	..	1·00631	1·00779
18·4	1·00730	(1·00730) basis	(1·00730) basis
19·0	1·00704	1·00726	1·00728
59·4	1·00547	1·00561	1·00640
62·1	1·00538	1·00552	1·00634
83·8	1·00482	1·00486	1·00596
95·3	1·00453	1·00457	1·00577
108·4	1·00434	1·00426	1·00557

February 6, 1902.

Sir WILLIAM HUGGINS, K.C.B., D.C.L., President, in the Chair.

His Royal Highness the Prince of Wales (elected June 8, 1893), was admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

- I. "The Stratifications of Hydrogen." By Sir WILLIAM CROOKES, F.R.S.
- II. "Radio-activity and the Electron Theory." By Sir WILLIAM CROOKES, F.R.S.
- III. "The Density and Coefficient of Cubical Expansion of Ice." By Dr. J. H. VINCENT. Communicated by Professor J. J. THOMSON, F.R.S.

- IV. "On the Increase of Electrical Resistivity caused by Alloying Iron with Various Elements, and the Specific Heat of those Elements." By Professor W. F. BARRETT, F.R.S.
- V. "Continuous Electrical Calorimetry." By Professor H. L. CALLENDAR, F.R.S.
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"The Stratifications of Hydrogen." By Sir WILLIAM CROOKES, F.R.S. Received January 9,—Read February 6, 1902.

The following pages give the outcome of attempts to prepare pure hydrogen, and experiments on the stratifications exhibited by the purified gas under the influence of an induction current. The researches were commenced in 1884 and have been continued intermittently to the present time.

The original apparatus consisted of a vacuum tube of soda-glass, 6 inches long and $\frac{1}{2}$ inch wide, having sealed-in aluminium terminals at each end. The pole at one end was ring-shaped, at the other pointed. The vacuum tube was connected to the mercury pump at one end, a tightly packed phosphoric anhydride tube intervening. At the other end was another phosphoric anhydride tube, a hydrogen generator of zinc and dilute sulphuric acid and a tap to control the flow of gas. The hydrogen was passed through the apparatus for some hours, and the whole was exhausted to a high point, re-filled, and again exhausted. This was repeated many times; but, on exhausting to the stratification-point, I could get no spectrum which did not show, in addition to hydrogen, also mercury.

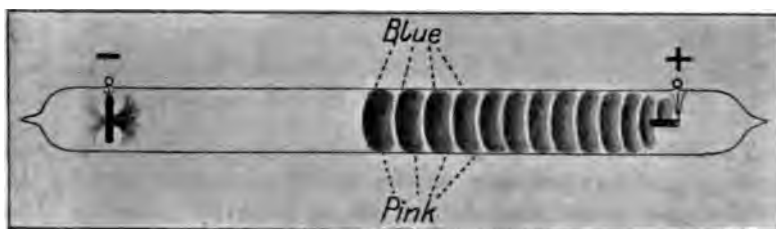
The apparatus was therefore modified. Strips of palladium foil were charged with hydrogen by the electrolysis of dilute sulphuric acid; a 4-cell Grove's battery being used for 1 hour. After drying, the palladium strips were put in a glass tube and sealed between the generator and vacuum tube. At first, crude gas from the generator was used to wash out the apparatus, and after many fillings and exhaustions—the last to the highest possible point—the generator and tap were sealed off, leaving only the palladium and drying tubes attached to the apparatus. A portion of the palladium was now gently heated; the gauge sank 12 cm., when it was again well exhausted and a little more hydrogen liberated. This was repeated three times, when the tube was exhausted to the stratification-point—about 4 mm.

Parti-coloured Stratifications.

The strata were twelve in number, and of a slightly concavo-convex button-shape, each of a blue colour on the convex side facing the negative pole, and pink on the other side. On reversing the current,

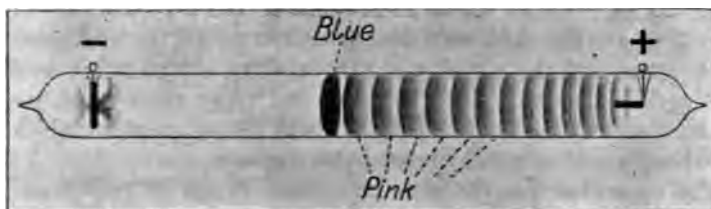
the buttons faced round, always presenting the blue face to the negative pole. Examination with a spectroscope showed strong hydrogen lines in the pink parts, and both hydrogen and mercury in the blue parts. Fig. 1 shows the appearance at this stage.

FIG. 1.



The exhaustion was now raised to 2 mm., when the whole of the blue faces of the parti-coloured buttons suddenly migrated to one bright blue, well-formed button, nearest the negative pole, all the other buttons remaining pink. The appearance is shown in fig. 2.

FIG. 2.



Round the negative pole an indistinct halo showed both mercury and hydrogen; but on the blue button mercury only was detected, not a trace of even the brightest hydrogen line being there seen. On the pink portions the hydrogen lines were in excess, but mercury could be seen all along the tube.*

* I have been unable to find any reference to this concentration of the blue constituents of the strata into one single button at the end near the negative pole. In the classical researches of Messrs. De la Rue and H. W. Müller on the 'Electric Discharge with the Chloride of Silver Battery,' numerous references are made to the blue and pink parti-coloured character of the strata, and also to the change of colour from all blue to all pink which follows a change in the electrical conditions. Thus we read:—"Thirty-nine strata, the convex side being blue and the broader concave side reddish." "Twenty-one strata, very blue on the convex face, pink on the concave."—"Phil. Trans., vol. 169, p. 175.

Sometimes the stratifications are described as all of one colour:—"Twenty-one very blue strata." "The strata were blue, and sixty-one in number." "Showing

At 15 mm. pressure very little hydrogen could be detected by the spectroscope, the stratifications had almost disappeared, and mercury was strong throughout the tube.

Elimination of Mercury Vapour.

The mercury apparently diffused into the tube from the pump and many devices were adopted to keep it out. Long glass tubes filled with purified sulphur broken into coarse pieces were partially successful, but something (probably sulphur) was communicated to the hydrogen which interfered with the purity of the colours. Also the sulphur did not prevent a little mercury diffusing in if the tubes were left on the pump all night.

Experience showed that mercury was very difficult to eliminate from a tube once it had gained access. It adheres to the walls of the tube, and defies detection in the cold, but becomes visible in the spectroscope as soon as the tube is heated by a lamp. Therefore a new hydrogen tube was made, and connected with tubes containing sulphur, bright copper turnings, and phosphoric anhydride; the copper intervening to keep out the sulphur. Here, again, mercury was detected after some days, but only in minute traces. In each of these

twenty-one double strata intensely blue, but with a carmine line between the components."—*Phil. Trans.*, vol. 169, pp. 175, 190.

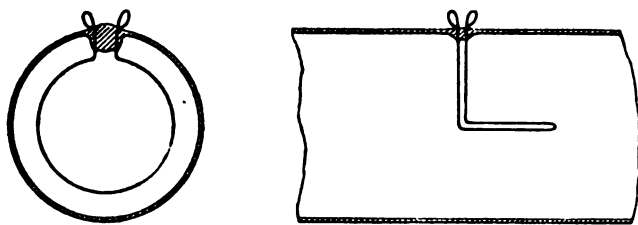
Frequently a change from all of one colour to all of the other colour is recorded. Thus:—"The tube was filled to within 1 inch of the negative with strata; all these were blue, but they turned pink when 200,000 ohms resistance was introduced. When 7,590,000 ohms resistance was introduced, a very close and somewhat agitated pink stratification was produced." "Thirty-four steady blue strata were produced; on introducing 200,000 ohms resistance, the strata turned pink." "With 3600 cells the strata were blue and sixty-one in number. With 700,000 ohms resistance the strata were reduced to eighteen and turned pink." "A change of current frequently produces an entire change in the colour of the strata. For example, in a hydrogen tube, from a cobalt-blue to a pink."—*Phil. Trans.*, vol. 169, pp. 183, 190, 231.

Very few spectrum observations were made by the authors, and they are not very definite in character. One tube, which gave ten luminosities, was examined with the spectroscope. "The C and F lines were brilliantly seen in the glow around the negative terminal, but were not visible in the spectrum of the luminosities, notwithstanding that they were brighter than the negative glow; there were blue, green, and red visible, but not the characteristic green and red lines of hydrogen." In another tube, "The characteristic hydrogen lines were very brilliant when the spectroscope was directed to the glow around the negative terminal, but quite a different spectrum was seen on a bright stratum, with mercury lines in the orange." "The hydrogen lines could not be seen either in the strata or the glow on the negative ring, but instead of them mercury lines came out strongly. There is reason to think that at this stage there was little gas except mercury vapour in the tube." "A change of current frequently . . . changes the spectrum of the strata; moreover, the spectra of the illuminated terminals and the strata differ."—*Phil. Trans.*, vol. 169, pp. 180, 216, 232.

cases occurred the phenomena of the pink and blue strata at one exhaustion, changing to all-pink with a concentration of the blue to the front on increasing the vacuum; the single blue button showing the mercury spectrum. Continued pumping diminished the hydrogen, but apparently did not affect the mercury to the same extent. It appears that the great diffusibility of the hydrogen causes it to be readily pumped out, whilst the mercury is continually being replenished by diffusion from the pump.

A slight difference is produced in the purity of the colours of the strata according as aluminium or platinum poles are used. A pair of vacuum tubes was made, one having the usual shaped aluminium poles, the other having platinum poles of a special construction. Each terminal was of double wire, at one terminal bent into the form of a ring, and at the other a straight pole. The ends of the wires forming the poles were sealed through the tube close together but not touching, and terminated in loops outside, so that they could be raised to red or white heat by connecting them with a few battery cells. The arrangement will be readily understood by reference to the accompanying drawings (fig. 3). Thus heat could easily be applied during exhaustion,

FIG. 3. (Full size.)

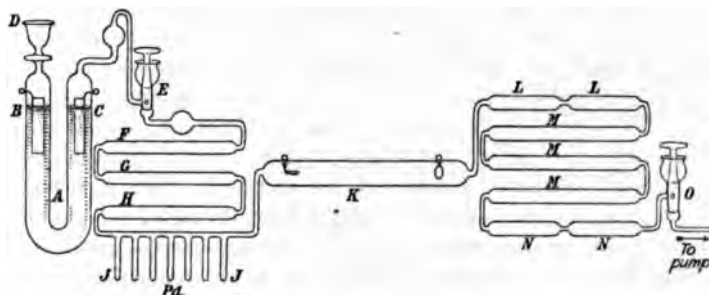


first to one pole and then to the other, even while the induction spark was passing. At first much gas was liberated from the platinum, but by repeated heating, pumping, and passing the spark, all the occluded gas was abstracted, and then the fillings with hydrogen and subsequent operations were commenced.

The general plan of the apparatus is shown in the drawing (fig. 4). At the end furthest from the pump is the hydrogen generator, A, consisting of a U-shaped tube filled with dilute sulphuric acid, having in one leg a plate of amalgamated zinc, B, and in the other a sheet of platinum, C. Both the platinum and the zinc are connected metallically to platinum wires sealed through the glass. A funnel with a stopper, D, sealed to the outer limb of the generator admits dilute acid when required. A tap, E, on the other limb enables the reservoir of hydrogen to be disconnected from the rest of the apparatus. Following this tap is a battery of three tubes, one F, containing small lumps of

dry caustic potash; the second, G, and the third, H, tubes containing phosphoric anhydride. Between the second phosphoric anhydride and the vacuum tube is another tube having sealed on to it, comb-like, seven projecting arms, J J, each containing a strip of palladium foil saturated with hydrogen.

FIG. 4.



The vacuum tube, K, is 8 inches between the terminals, and $\frac{3}{4}$ inch diameter; it comes next to the comb, and then between it and the pump is a battery of tubes, each 12 inches long, to keep out the mercury. The first tube, L, is divided by a constriction in the middle, and contains, in the half next the vacuum tube, bright metallic copper, in the other half sulphur. The three next tubes, M, M, M, contain sulphur, but in the middle of each are placed a few grains of iodine separated from contact with the sulphur by a plug of asbestos on each side. The sulphur is prepared by keeping it fused at a temperature a little below its boiling-point till bubbles cease to come off, so as to get rid of water and hydrogen compounds. It is then allowed to cool, and is pounded and sifted so as to get it in the form of granules, averaging a mm. in diameter. Ignited asbestos is packed at each end of the tubes to keep the contents from blowing out when the vacuum is proceeding, or air is suddenly let in.* Next follows a tube, N, N, constricted in the middle, containing in the first half phosphoric anhydride, and in the second finely powdered dry caustic potash. A tap, O, connects the apparatus with the pump, to prevent diffusion of mercury when the pump is not in use. All parts of the apparatus were built up in place and sealed together with the blow-pipe. The glass was new, and the apparatus had been kept apart from mercury until it was sealed together.

The apparatus was exhausted from air, the tap E being closed and

* An apparatus of this kind was briefly described by the author in a paper read before the Royal Society in 1885 ("Radiant Matter Spectroscopy—Part II, Samarium," 'Phil. Trans.,' vol. 176, p. 698, June 18, 1885). The apparatus was used to prevent mercury from getting into the small radiant-matter tubes employed in the research.

D open. Electrolysis was then commenced (D being closed), and the tap E was slightly turned until the escape of hydrogen into the apparatus was equal to the speed of its generation. The apparatus was filled, and several times exhausted, until no improvement in the spectrum or stratifications could be seen. The electrolytic cell was then sealed off at a narrow constriction between the first potash tube, F, and the phosphoric anhydride tube, G. After good exhaustion one of the branch tubes of palladium was heated, when the gauge sank several centimetres. Exhaustion and re-filling from fresh palladium were repeated until no alteration was detected in the appearance of the strata. Then, for the first time, I obtained hydrogen strata showing no blue, either throughout the tube or concentrated in front, whilst the most careful examination showed no mercury. The stratifications were all pink, and showed the hydrogen lines strongly.

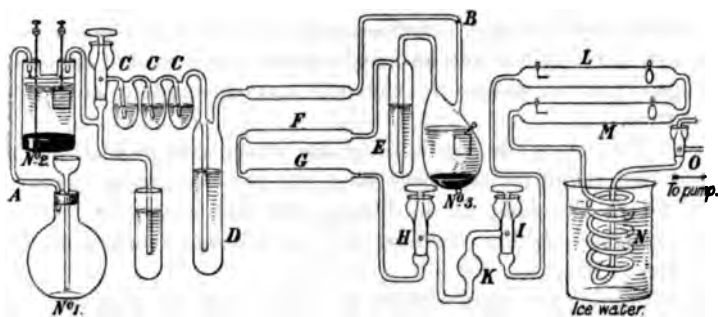
Many disadvantages were noticed in the apparatus just described, the chief being the danger of introducing more impurities than were kept out by the copper, sulphur, and iodine tubes. The palladium method of introducing hydrogen was not altogether satisfactory, as only small quantities could be dealt with, and occasionally at a critical point the store was exhausted. Also, the electrolytic generator of hydrogen was too small. It was decided, therefore, to devise and fit up an entirely new piece of apparatus. In this another method was used for keeping out the mercury. It had been noticed that the diffusion of mercury from the pump proceeded the more slowly as the distance from the pump and the narrowness of the connecting tubes increased. It was thought that by introducing a long narrow spiral between the pump and the apparatus, one complicated system of tubes, with their attendant dangers, could be removed; the result showed this supposition to be correct. Two vacuum tubes were employed, one having aluminium the other platinum terminals. The hydrogen generators were increased in size and number, and were so distributed that they could be sealed off one after the other during the progress of the experiment.

Stratifications in Pure Hydrogen.

The arrangement of the apparatus is shown in fig. 5. The three hydrogen generators are called Nos. 1, 2, and 3. In No. 1, the gas is generated by the action of hydrochloric acid on zinc. This crude hydrogen is only used to drive out the air from the rest of the apparatus and to remove the air dissolved in the liquids. When it had done its work, the generator was sealed off between Nos. 1 and 2, at A. It was considered that having the apparatus to begin with full of even somewhat impure hydrogen was better than starting with it full of air. The second and third generators contain at the bottom a pasty amalgam of mercury and zinc forming one pole, and a piece of

platinum forming the other pole ; the electrolyte is dilute hydrochloric acid. Platinum wires sealed through the sides of No. 3 carry the current from three Grove's cells to the interior. After the apparatus has had generator No. 1 removed, a large quantity of hydrogen is passed through from the second generator, with the object of replacing the impure hydrogen by some of a purer quality. When No. 2 is exhausted, it also is sealed off at B, leaving only the third generator with its drying tubes connected with the apparatus. Before sealing off No. 2, filling and exhausting is carried on until the hydrogen shows no impurity when spectroscopically examined in a capillary tube attached to the vacuum tube. The gas from the first and second generators bubbles first through strong caustic soda, C, C, C, to remove any acid

FIG. 5.



carried over from the generators, then through strong sulphuric acid, D, to take away the bulk of the moisture, and thus save the drying tubes ; it then passes through the purifying arrangements more especially connected with the third generator. Having sealed off Nos. 1 and 2, gas is evolved from No. 3 generator. Hence it passes through strong sulphuric acid in the tube H ; then over a tube filled with granulated caustic soda, F ; and next through a tube, G, tightly packed with phosphoric anhydride. H and I are two taps, having a reservoir, K, between them. When full of gas, H and I are closed, and the tubes L and M, after having been exhausted to a high point, can then be fed with limited amounts of pure dry hydrogen by slightly opening tap I, and closing it when equilibrium is restored between L, M, and K. N is a spiral of narrow glass tubing immersed in a beaker of ice and water. At O is a tap to keep mercury from diffusing into the pump if the apparatus has to be left all night. The vacuum tube, L, is provided with aluminium poles, and the tube M has the platinum poles made double for heating purposes, as shown in fig. 3.

Hydrogen from the first generator was passed through the apparatus for two hours, when it was sealed off. The whole apparatus was

exhausted to a high point, and No. 2 generator was set to work. Hydrogen was passed several times at full pressure through the apparatus for one or two hours, and then exhausted to the stratification point. During these operations the platinum terminals of one of the vacuum tubes were heated to full redness, and the current was kept on both tubes for some hours to drive off occluded gases.

Finally the second generator was sealed off, and hydrogen used from the remaining generator. After much washing out with hydrogen at the ordinary pressure, exhaustion and re-filling were continued, and finally the reservoir K was filled, both taps, H and I, being closed. The tubes were highly exhausted to the non-conducting point, and tap I opened and then closed, so as to introduce a little hydrogen. H was then opened and again closed, so as to equalise the pressure in I, and exhaustion proceeded to the stratification point. At first the strata were irregularly coloured with a suspicion of blue on one face, but as the operations just described were continued, the blue faces disappeared, the stratifications assumed a pure pink hue, and showed the hydrogen spectrum alone; no mercury was detected in any part of either tube.

From the first to the eighth filling the strata were pink with a trace of slaty blue colour on the faces next the negative pole. From the tenth filling the blue faces disappeared, and after the twentieth filling no trace of blue could be seen, and the spectrum of hydrogen alone was visible.

On examining the spectra of the stratified gas in the two tubes, each showed strongly the line spectrum of hydrogen; but while the spectrum in the platinum-poled tube showed pure red, blue, and green lines on a black ground, that in the aluminium-poled tube showed in addition the fainter hydrogen line spectrum in the yellow and orange. This result may be due to the greater surface exposed by the aluminium poles; it was not further examined.

Having at last succeeded in getting hydrogen free from mercury, experiments were instituted to verify the inference that the blue components of the blue and pink strata usually attributed to hydrogen were really due to the presence of a trace of mercury.

Origin of the Blue Component of Parti-coloured Stratifications.

I used an apparatus similar to the last, but with only one generator. If my idea was correct, that the mercury in the course of a few hours diffused into the hydrogen tube from the pump when it was not at work, there ought to be an access of blue faces to the pink buttons after the exhausted apparatus had been at rest. After filling with hydrogen and exhausting several times, a hydrogen vacuum was obtained showing no blue faces to the pink strata. The apparatus was

then left all night, and the stratifications examined next morning. The blue colour to each face was now unmistakably visible. The refilling with hydrogen and exhausting was then continued. It was not possible in this way to get the tube entirely free from mercury, although it got less and less, as shown by the diminution of the blue faces.

Occasionally, when no mercury was present, a faint blue edging to some of the front pink strata was seen. This blue was too faint to show lines in its spectrum. After much searching the blue tint was traced to the phosphoric drying tubes. A clean tube was taken for stratifications, and sealed to the apparatus used in the last experiments. The whole was exhausted to a high point, and one of the phosphoric anhydride tubes was gently heated with a gas flame, the current kept going. Instantly a flood of blue light swept through the tube, and when concentrated in a narrow constriction the light showed a complicated spectrum which was not recognised: none of the characteristic lines of the phosphorus spectrum could be seen in it. The tube was cleared of the blue colour by introducing hydrogen and pumping it out a few times, and then hydrogen was introduced and exhaustion continued to the stratification point. The strata now were pink with no appearance of blue. Warming the phosphoric anhydride tube at once reproduced the faint blue edging to the pink discs. This blue colour was different both in tint and intensity to the blue colour produced by mercury, but it was too faint to show a spectrum except in the constricted part.

It is of importance to ascertain whether the body producing this blue colour can be removed from the phosphoric anhydride. The drying tube was again heated to the subliming point of the anhydride, hydrogen passed in, and the pump worked until the vacuum was almost non-conducting. The heating, passing in hydrogen, and pumping were several times repeated, the impurities diminishing each time. Ultimately a point was reached when, the tube being non-conducting, heating the phosphoric anhydride did not communicate any gas to the vacuum tube. At this stage the phosphoric anhydride still retained unimpaired its affinity for water. In any accurate experiment, therefore, the phosphoric anhydride tubes should have a preliminary heating in a vacuum to eliminate the impurity. This may be done with several tubes at a time, when they can be sealed at each end and preserved for future use.

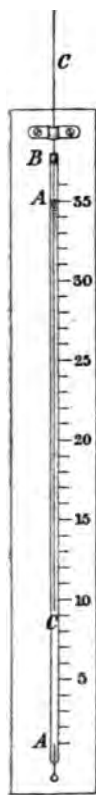
It is thus seen that this blue glow is due to some impurity in the phosphoric anhydride. Likewise I have shown from the examination of its spectrum that it is not due to phosphorus. The glow probably is due to some intermediate oxide of phosphorus. In any accurate work with the mercury pump, where phosphoric anhydride is used as the drying agent, this source of impurity must not be overlooked.

An addition to the apparatus was made, a supplementary tube sealed on containing a grain of corrosive sublimate. This was used as being non-volatile at the ordinary temperature, but easily vaporised by heat. The experiment last described was continued, and immediately after the phosphoric blue edge appeared fresh hydrogen was let in and exhaustion continued till the faint blue was eliminated. The mercury salt was then heated, when immediately a rich blue edging appeared on the face of each pink stratification and the yellow lines of mercury shone out distinctly. Mercury blue is of a fuller colour than that of the phosphoric blue.

Altering the Colour of Stratifications.

In the 'British Association Reports' for 1865 (Abstracts, p. 15), Mr. Gassiot describes the changes produced in the colours of the stratifications by introducing a water resistance in series with the vacuum tube. Having shown that the blue components of the

FIG. 6.



the stratifications are due to the presence of a trace of mercury with the hydrogen, experiments were commenced to ascertain what difference in the strength of the induced current would be necessary to alter the relative intensities of the pink and blue strata. Accordingly, I fitted up a resistance, shown in fig. 6, consisting of a glass tube, A A, 3 feet long and $\frac{3}{8}$ inch diameter nearly filled with distilled water. Through a cork, B, at the upper end of the tube a copper wire, C C, passes, and by raising or lowering the wire either the whole resistance of the water or any fraction of it can be thrown into the circuit. The upper part of the wire is connected with one pole of the coil and the water is connected with the other pole by means of a small platinum wire sealed through the bottom of the tube.

The wire was pushed down until it touched the platinum at the bottom, thus cutting out the water resistance. The strength of hammer-spring and the exhaustion were arranged to show good pink and blue discs. The wire was then gradually withdrawn, when the blue components gradually faded, and at a resistance of 6 inches of water the stratifications were all pink. Spectroscopic examination showed that in the parti-coloured state mercury was strongly present in all the blue components, together with the C hydrogen line; the mercury spectrum, however, being in excess. But when the water resistance was put in, and the buttons were all pink, mercury was still to be detected, but the hydrogen spectrum was more prominent. The green line of mercury was always the first to appear,

and when the hydrogen became visible the first line to appear was the red.

That the change from parti-coloured stratifications to all pink is occasioned by an alteration in the intensity of the spark, is also shown by the fact that altering the make and break of the coil produces the same change as putting in the water resistance. Screwing up the hammer-spring so as to induce strong magnetism in the core and high potential of spark, gives pink and blue parti-coloured buttons. Weakening the spring, and thus weakening the magnetism, gives buttons which are all pink. I cannot, however, in this way cause the migration of the blue buttons to one single button in front. This phenomenon is produced only by an alteration of the exhaustion. Conversely, an alteration of the exhaustion will not change parti-coloured buttons to buttons of all one colour; this requires a change in electrical energy.

I consider the water resistance acts thus:—At the same rarefaction, hydrogen conducts the current better than mercury vapour. With a strong induction-spark at a certain degree of exhaustion, the conducting power of the rarefied hydrogen is not sufficient to carry the whole of the current; some of it, therefore, is conducted through the mercury vapour, when the intensity of the blue drowns the feebler colour of the hydrogen. When, however, a resistance of 6 inches of water (equal to about 500,000 ohms) is inserted, the current is so weakened that the hydrogen can carry the whole of it, and the blue of the mercury is not seen.

Conclusions; Chiefly Theoretical.

The phenomenon of blue faces on the pink discs is probably due to some such action as the following:—At the exhaustion necessary to give stratifications, there is a wide dark space round the negative pole. Here the negative electrons (Radiant Matter), issuing from the pole with enormous velocity, have sufficient energy to clear a space in front of them to a distance varying with the degree of exhaustion.

Dr. A. Schuster considers that the discharge through mercury vapour in a vacuum tube, when quite free from air, will not give rise to stratifications, nor to the dark negative space.* My own experiments† show that the dark space will form in pure mercury vapour. Whichever view may be correct, there is no doubt that if stratifications in mercury vapour are not altogether unknown, they are much more difficult to produce than similar phenomena in hydrogen or other diatomic gases. At a certain critical stage of the exhaustion, when

* Dr. A. Schuster, "Experiments on the Discharge of Electricity through Gases," 'Roy. Soc. Proc.,' vol. 37, p. 318.

† 'Journ. of the Inst. Electrical Engineers,' vol. 20, p. 41.

both hydrogen and mercury are present, I obtain both mercury and hydrogen strata.

The following experiments appear to show that the presence of hydrogen* has a considerable influence on the behaviour of mercury vapour in vacuum tubes. A tube with aluminium terminals had been attached to the pump for several days, and consequently mercury had diffused in and condensed on its sides. The tube was exhausted until, under the electric discharge, it phosphoresced of a brilliant green and was near the non-conducting point. It was heated very strongly with a Bunsen flame, when a faint pink luminosity was observed between the poles, the spectrum of mercury being absent. The tube was allowed to cool and a little hydrogen was introduced, when the usual pink stratifications appeared. On heating the tube to about the same temperature as before blue faces showed on the pink buttons, and the mercury spectrum was seen throughout the tube, especially in the neighbourhood of the negative pole.

It is known that in a vacuum tube, at an exhaustion approaching the stratification-point, any slight obstruction, such as constriction in the tube, or a series of wires sealed in, will cause luminous strata to hang round the obstruction. In a similar way, the hydrogen strata afford an anchorage, as it were, for the mercury, each hydrogen luminosity having a little blue glow of mercury hanging on to it; whereas, were there no hydrogen, no mercury stratifications would be seen.

The pink and blue luminosities show where the electrons and gaseous atoms meet; when the speed of the electrons is suddenly diminished, the shock throws the atom into greater vibration, which, being communicated to the ether, produces vibrations of definite wavelengths, constituting the special spectrum of the atom. The dense mercury atom is not driven back so much as the lighter hydrogen atom—hence the blue front to the pink buttons. A very little difference in the exhaustion suffices to break the adhesion between the mercury and the hydrogen; then the mercury vapour diffusing along the tube meets the electrons from the negative pole and is swept back to the head of the hydrogen strata, and becomes apparent as a single button of blue light.

Radiant Matter. Electrons.

I have spoken of "Radiant Matter" and "Electrons" as if they were identical. Nearly twenty-five years ago I was led by experiments in highly rarefied tubes to assume the existence of matter in an *ultra-gaseous* state. Later, in a lecture delivered before the British Association at the Sheffield Meeting, 1879,† I first used the

* Possibly another gas would do as well. Hydrogen was used because in this instance the hydrogen generator was attached to the tube.

† 'Chemical News,' vol. 40, pp. 91, 104, 127.

expression "Radiant Matter," or matter in the ultra-gaseous state, to explain the novel phenomena of phosphorescence, trajectory, shadows, mechanical action, magnetisation, and intense heat. "In studying this fourth state of matter," I said, "we seem at length to have within our grasp and obedient to our control, the little indivisible particles which with good warrant are supposed to constitute the physical basis of the universe. We have seen that in some of its properties radiant matter is as material as this table, whilst in other properties it almost assumes the character of radiant energy. We have actually touched the borderland where matter and force seem to merge into one another."*

In twenty-five years one's theories may change, although the facts on which they are based remain immovable. What I then called "Radiant Matter" now passes as "Electrons," a term coined by Dr. Johnstone Stoney, to represent the separate units of electricity, which is as atomic as matter. What was puzzling and unexplained on the "Radiant Matter" theory is now precise and luminous on the "Electron" theory. Thus my early hypotheses fall into order by the substitution of one expression for the other. A chemical ion consists of a material nucleus or atom of matter constituting by far the larger portion of the mass, and a few electrons or atoms of electricity. The electrons are the same as the "satellites" of Lord Kelvin and the "corpuscles" or "particles" of J. J. Thomson.

Electrons probably leave the negative pole with a velocity nearly uniform, modified to a considerable extent by the degree of exhaustion, and to a less extent by the electromotive force behind them. Many experiments—the details I must leave to a future occasion—show that the liberated electrons do not behave as a gas, *i.e.*, they have not properties dependent on inter-collisions, mean free path, &c. ; they act more like a fog or mist, are mobile and carried about by a current of air to which they give temporary conducting powers, clinging to positively electrified bodies and thereby losing mobility, and settling on the walls of the containing vessel if left quiet.

On the other hand, the crowd of hydrogen or mercury atoms, by virtue of molecular motion and inter-collisions, act as gases. Whilst their *mean* free paths are conditioned by the degree of exhaustion, there may be amongst them a certain number of *actual* free paths differing widely on each side of the mean. Under the influence of the electromotive force, and at the right degree of exhaustion, these atoms arrange themselves in groups,† while the rushing swarm of electrons

* 'Chemical News,' vol. 40, p. 130.

† In an address delivered before the Institution of Electrical Engineers, January 15th, 1891, I gave an outline of a theory of stratifications in rarefied gases. The following quotation renders my meaning clear:—"If, in any much frequented street, at some time when the stream of traffic runs almost equally in both directions, we take our stand at a window from which we can overlook the passing

driven from the negative pole meet them and render them visible. According to J. J. Thomson the mass of an electron is about the 1/700th part of that of the hydrogen atom, and as these masses start from the negative pole in a vacuum tube with a velocity of the order of half that of light, it is easy to see that their heating, phosphorescent, and mechanical power must be stupendous.

The basis of the Electron, as I foreshadowed in 1879 in the case of Radiant Matter, is probably the same in all cases—the Protyle* from which the chemical atoms were assumed to be formed.

On the two-fluid theory, the electrons constitute free negative electricity, and the rest of the chemical atom is charged positively, although a free positive electron is not known. It seems to me simpler to use the original one-fluid theory of Franklin, and to say that the electron is the atom or unit of electricity.† Then a so-called

crowd, we shall notice that the throng on the footway is not uniformly distributed, but is made up of knots—we might almost say blocks—interrupted by spaces which are comparatively open, we may easily conceive in what manner these knots or groups are formed: some few persons walking rather more slowly than the average rate slightly retard the movements of others, whether travelling in the same or in an opposite direction. Thus a temporary obstruction is created. The passengers behind catch up to the block and increase it, and those in front, passing on unchecked at their former rate, leave a comparatively vacant space. If a crowd is moving all in the same direction, the formation of these groups becomes more distinct. Hence mere differences in speed suffice to resolve a multitude of passengers into alternating gaps and knots. Instead of observing moving men and women, suppose we experiment on little particles of some substance, such as sand. If we mix the particles with water in a horizontal tube and set them in rhythmical agitation, we shall see very similar results, the powder sorting itself with regularity into alternate heaps and blank spaces. If we pass to yet more minute substances, we observe the behaviour of the molecules of a rarefied gas when submitted to an induction current. The molecules here are free, of course, from any caprice, and simply follow the law I seek to illustrate, and though originally in a state of rampant disorder, yet under the influence of the electric rhythm, they arrange themselves into well-defined groups or stratifications.”—‘*Journ. of the Inst. Elect. Engineers*,’ vol. 20, p. 10.

* ‘*Report of the Fifty-sixth Meeting of the British Association*,’ Birmingham, 1886, p. 568 Address to the Chemical Section.

† “The theory of definite electrolytical or electro-chemical action appears to me to touch immediately upon the *absolute quantity* of electricity or electric power belonging to different bodies. . . . And when comes the fact that the electricity which we appear to be capable of loosening from its habitation for a while, and conveying from place to place, *whilst it retains its chemical force*, can be measured out and being so measured, is found to be *as definite in its action as any of those portions* which, remaining associated with the particles of matter, give them their *chemical relation*; we seem to have found the link which connects the proportion of that we have evolved to the proportion of that belonging to the particles in their natural state.”—Faraday’s ‘*Experimental Researches in Electricity*,’ par. 852.

“The equivalent weights of bodies are simply those quantities of them which contain equal quantities of electricity; . . . it being the ELECTRICITY which

negatively charged chemical atom is one having a surplus of electrons, the number depending on the valency, whilst a positively charged atom is one having a deficiency of electrons. Differences of electrical charge may thus be likened to debits and credits in one's banking account, the electrons acting as current coin of the realm.

"Radio-activity and the Electron Theory." By Sir WILLIAM CROOKES, F.R.S. Received February 4,—Read February 6, 1902.

Electrons emanating from radio-active bodies behave like material particles, and are impeded by the molecules of the surrounding medium, in contrast with ether waves, which are not thus affected except by absorption. It is not difficult to put these indications to test. A pair of shallow cells, A B (fig. 1), 1.5 mm. deep and 25 mm. square, were made by cementing slips of glass to a thick glass plate.

determines the equivalent number, because it determines the combining force. Or if we adopt the atomic theory or phraseology, then the atoms of bodies which are equivalents to each other in their ordinary chemical action have equal quantities of electricity naturally associated with them."—*Ibid.*, par. 869.

"In former investigations of the action of electricity it was shown . . . that the quantity of electric power transferred onwards was in proportion to, and was definite for, a given quantity of matter moving as anion or cation onwards in the electrolytic line of action; and there was strong reason to believe that each of the particles of matter then dealt with, had associated with it a definite amount of electrical force, constituting its force of chemical affinity."—*Ibid.*, par. 1707.

(In all the above quotations the italics and capitals are Faraday's.)

"It is therefore extremely natural to suppose that . . . every molecule of the cation is charged with a certain fixed quantity of positive electricity, which is the same for the molecules of all cations, and that every molecule of the anion is charged with an equal quantity of negative electricity."—Clerk Maxwell's 'Treatise on Electricity and Magnetism,' First Edition, vol. 1, 1873, p. 308.

"This definite quantity of electricity we shall call the molecular charge. If it were known, it would be the most natural unit of electricity."—*Ibid.*, p. 311.

"Suppose . . . we call this constant molecular charge, for convenience in description, *one molecule of electricity*."—*Ibid.*, p. 312. (The italics are Maxwell's.)

"Nature presents us with a single definite quantity of electricity . . . For each chemical bond which is ruptured within an electrolyte a certain quantity of electricity traverses the electrolyte, which is the same in all cases."—G. Johnstone Stoney, "On the Physical Units of Nature," British Association Meeting, 1874, Section A, 'Phil. Mag.,' May, 1881.

"The same definite quantity of either positive or negative electricity moves always with each univalent ion, or with every unit of affinity of a multivalent ion."—Helmholtz, Faraday Lecture, 1881.

"Every monad atom has associated with it a certain definite quantity of electricity; every dyad has twice this quantity associated with it; every triad three times as much, and so on."—O. Lodge, "On Electrolysis," 'British Association Report,' 1885.

The cells were filled to the same depth with a radio-active substance chiefly containing actinium.* Over cell A was placed a piece of thick lead pipe, 28 mm. high and 25 mm. internal diameter, to insure that any emanations from the active substance in A would be confined to the inside of the hollow cylinder. The radio-active substance in B

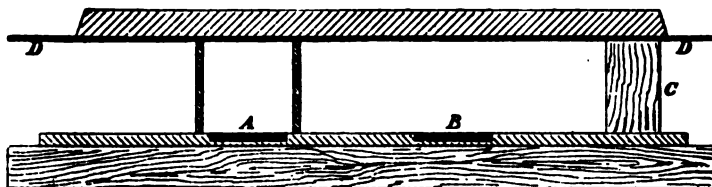


FIG. 1.—Elevation.

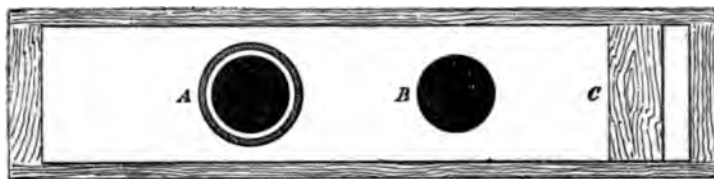


FIG. 1.—Plan.

was freely exposed to the air, save for a pillar of lead at C, to support the sensitive film. A sensitive film was laid horizontally over the cylinder and support C. On the film was a plate of glass, and cylinder and film were pressed together by heavy weights. The whole was covered in a light-tight box and put in a dark cupboard.

At the end of 48 hours the film was removed and developed. There was a strong action shown over cell A (the one covered by the lead cylinder), but over B, the cell exposed to the air, there was no visible impression. Measured in Mr. Chapman Jones's "Opacity Meter" † the results were :—

Image over cylinder—opacity $\log. \ddagger = 0.79$; opacity $\S = 6.17$.

The experiment was repeated, using the same apparatus but a different preparation of actinium. In this case the exposure was for

* The body I called Uranium X in my Royal Society paper, May 10, 1900, has since proved to be M. Debierne's Actinium.

† 'The Photographic Journal,' vol. 20, p. 86, December 21, 1895.

‡ The opacity logarithm represents the density of the image, absolute density being represented by 2.00

§ The "opacity" is the whole number corresponding to the "opacity log." The "opacity" is directly proportional to the photographic energy acting on the sensitive surface.

72 hours. As before, there was a strong impression over cell A and none over cell B. The figures were :—

$$\text{Opacity log.} = 0.89; \text{Opacity} = 7.71.$$

These experiments indicate that the electrons from the radio-active agent, chiefly actinium, partake of the properties of a fog or mist of material particles, capable of diffusing away in the free air like odoriferous particles, when not kept in by a thick metal screen.

A further experiment was now tried with the same apparatus, the agent a strongly active radium and barium bromide. This material being self-luminous, a sheet of black paper was placed immediately over it, so that nothing but emanations capable of passing through the opaque paper would be subject to experiment. After 4 hours' exposure in total darkness, the film was developed. A good circular patch was obtained over cell A, and a faint diffused darkening showed over the rest of the film, darker at the spot immediately over cell B, fading away at the sides as the distance became greater. That this action was due to the material in the open cell B, and not to general fog over the plate, was seen by the clearness of the film where covered by the lead, and where shadows were thrown by the lead cylinder and pillar.

Circles of the same diameter were drawn round the dark impression over A, and round the darkest part of the impression over cell B. Measurements were taken of different parts of the spaces enclosed in these circles, and the mean of all these came out—

$$\text{Circle over cell A—Opacity log.} = 0.53; \text{Opacity} = 3.39.$$

$$\text{Circle over cell B—Opacity log.} = 0.32; \text{Opacity} = 2.09.$$

$$\text{Ratio B/A} = 0.62$$

The experiment was repeated with the addition of a sheet of aluminium, 0.02 mm. thick, under the black paper, the electrons now having to pass through both paper and metal before reaching the film. The exposure was for 6 hours, and the appearance on development was very similar to the last: a dark disc over the protected cell A, and a diffused action over the other part of the film, except in the shadow of the lead supports. Measurements as on the previous occasion gave the following results :—

$$\text{Circle over cell A—Opacity log.} = 0.78; \text{Opacity} = 6.03.$$

$$\text{Circle over cell B—Opacity log.} = 0.48; \text{Opacity} = 3.02.$$

$$\text{Ratio B/A} = 0.5.$$

A third experiment was tried with the same apparatus, using only the aluminium plate as a screen to cut off the luminous rays. The appearance on development was similar to the others, and the measurements of opacities were—

Circle over cell A—Opacity log. = 0.59 ; Opacity = 3.89.

Circle over cell B—Opacity log. = 0.29 ; Opacity = 1.95.

Ratio B/A = 0.5.

Finally, I tried polonium subnitrate, which gives off emanations hardly capable of passing through any screen, and greatly obstructed by a few centimetres of air.

The apparatus was substantially the same as the one just described, with the modification that the lead cylinder was 12 mm. high, and at the other end a rod of glass 12 mm. high was used to support the film. The reduced height was chosen, experience showing that polonium emanations have great difficulty in penetrating many millimetres through air. The exposure was 7 days, at the end of which time the film was developed. Over cell A a dark disc sharply defined the inside of the cylinder, while over cell B was a hazy diffused patch which to the eye looked much the fainter of the two. But measurements of patch A and of a disc over cell B of the same size as A, showed that the opacities in each case were practically identical, as shown by the following figures—

Circle over cell A—Opacity log. = 0.74 ; Opacity = 5.49.

Circle over cell B—Opacity log. = 0.76 : Opacity = 5.75.

Ratio B/A = 1.05.

A repetition of the experiment, taking the mean of five concordant results, gave the same opacities as before.

Without proving that the emanations from polonium are less material than those from actinium and radium, this experiment shows that their behaviour is entirely different as regards diffusibility through air. Whether this is due to the larger mass of the individual particles, or to the less distance they have to travel (12 mm. as against 28 mm. in the case of actinium and radium), or to some other cause, further experiments must decide.

M. and Mme. Curie have shown that radio-activity is communicable from radium and actinium compounds to bodies such as lead, copper, glass, ebonite, and paraffin. After exposure to emanations from the radium compound, these bodies have the property of communicating temporary conductivity to the air and other gases, and of thereby discharging an electrified body.

When such charged bodies are exposed to the air, in a single day they lose the greater part of their activity. These phenomena are observed with different radio-active salts of radium, also with salts containing actinium ; but polonium compounds, even when very active, do not communicate the effect.

Dr. Rutherford shows that air which has remained for some time in *the neighbourhood* of thorium and then is carried in a current to a

distance, retains its property of communicating radio-activity to other bodies. He explains these phenomena by supposing that thorium gives off a special kind of emanation capable of being conveyed by the air, and that this is the cause of the induced radio-activity.

To ascertain if the electrons or corpuscles from radium also possess the property of being carried along in a current of air, I fitted up an apparatus shown in fig. 2. A, B, and C are three brass tubes closed

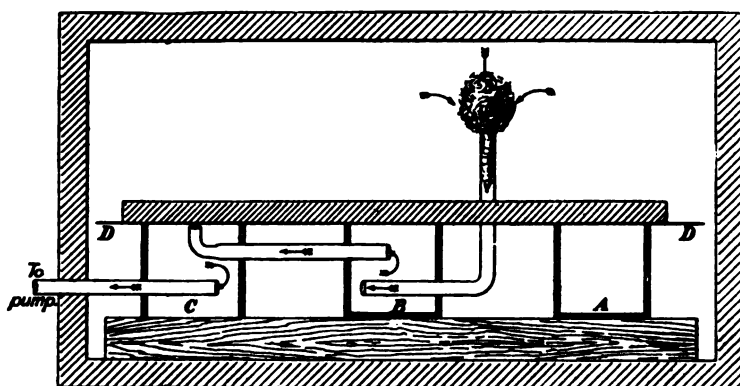


FIG. 2.—Elevation.

at the lower end and cemented with paraffin to a wooden block. The upper ends were accurately ground to a level surface, and then coated with a thin layer of paraffin wax. Holes were drilled in B and C, to admit glass tubes, cemented air-tight into the cylinders, as shown in the figure. The upper end of the tube in B was closed with a plug of cotton wool, and the outer end in C was connected to a water-pump, so that when the cylinders were closed at the top a current of air was drawn through B and C. As the radium compound was self-luminous, discs of thin aluminium foil were placed over cylinders A and B to cut off the luminous rays. A sensitive film was laid on the three cylinders over the aluminium, and it was tightly pressed down by a heavy weight; the contact between the film and the tops of the cylinders being sufficient to make the whole air-tight. At the bottom of A and B a radium compound was placed, equal weights and equal surface in each. The whole was put into a light-tight box, and air drawn through. The cylinder A was used only as a standard. The air passing into B was expected to carry along with it some of the corpuscles emitted from the active material at the bottom; and the inlet tube in C was turned up at the end, so that the stream of corpuscles-laden air should impinge on the surface of the centre of the film on C, and if it carried with it any radio-active properties the result should be seen on development,

by the production of a dark patch. If, however, the air carried no corpuscles there would be no image on the sensitive film over C.

The experiment was continued for 11 hours, 120 litres of air having passed through in the time.

On development and measuring the resulting images the following figures were obtained:—

Circle over cell A—Opacity log. = 0.342; Opacity = 2.20.

Circle over cell B—Opacity log. = 0.178; Opacity = 1.51.

Circle over cell C—Opacity log. = 0.025; Opacity = 0.11.

Ratio B/A = 0.68.

The apparatus was slightly altered. Cells B and C were put nearer together, and a short wide brass tube replaced the turned up glass tube formerly connecting them. The experiment was continued for 48 hours, during which time about 500 litres of air passed through the apparatus.

This repetition, allowing longer time, gave measurements as follows:—

Circle over cell A—Opacity log. = 0.964; Opacity = 9.20.

Circle over cell B—Opacity log. = 0.730; Opacity = 5.37.

Ratio B/A = 0.58.

In this experiment a slight but measurable darkening took place over cell C, shown by an opacity log. of 0.025 and an opacity of 0.11. This shows that some few corpuscles have been able to pass through the wide tube from B to C, and act on the photographic film.

It thus appears that a current of air passed over the surface of a radium compound carries with it a certain proportion of the corpuscles. This is proved by the diminished photographic action in the second cell, slightly confirmed by the evidence that some few of the corpuscles so carried away get to the sensitive film on cell C. Judging from our slender knowledge of the properties of free electrons, it is highly probable that they will not easily turn a corner, but cling to the sides of the tube through which they are being led. On the other hand, the constant collisions with the atoms of air may reduce their initial mobility almost to a vanishing point before they have travelled along the tube between B and C, and then they would be carried along with the air.

The experiment was repeated, using a preparation of actinium (Uranium X). It was kept going for 72 hours, during which time 750 litres of air were drawn through the apparatus. On development and measurement the following results were obtained:—

Circle over cell A—Opacity log. = 0.99; Opacity = 9.78.

Circle over cell B—Opacity log. = 0.67; Opacity = 4.68.

Circle over cell C—Opacity log. = 0.25; Opacity = 1.78.

Thoria prepared by the ignition of Dr. Knöfler's highly purified thorium nitrate* was now tested in a similar apparatus to the one last described, the only alteration being the removal of the covering aluminium plates, thoria not being self-luminous. The experiment was continued for 108 hours, 1125 litres of air being drawn through the cells during the time.

Measurements of the developed images gave the following results:—

Circle over cell A—Opacity log. = 0.306 ; Opacity = 2.02.

Circle over cell B—Opacity log. = 0.260 ; Opacity = 1.82.

Circle over cell C—Opacity log. = 0.046 ; Opacity = 0.10.

Here also the results agree with those tried with radium and actinium compounds, that corpuscles are carried by a current of air from cell B, through the connecting tube to cell C. They also confirm those of Dr. Rutherford—who finds that thorium emanations travel in a current of air while retaining their activity—and of P. Curie and A. Debierne, who show that induced radio-activity can be transmitted through capillary tubes of an internal diameter of 0.1 mm. and 75 cm. in length, bent once at right angles.

I have not obtained, however, a similar result with the emanations from hydrogen peroxide. As shown by Dr. Russell, this substance has a strong action on a sensitive photographic plate. The emanation from a bottle half full of hydrogen peroxide acts strongly on a sensitive film laid over the open mouth of the bottle for 24 hours, while there is no action in 72 hours if a U-shaped tube passed through the cork of the bottle and the sensitive film is put close to the open end of the tube. Dr. Russell tells me his observations confirm my experiments.

A highly active self-luminous radium compound loses some of its power on long exposure to the ordinary air of the laboratory. Ignition to red-heat restores, however, its self-luminosity, and when sealed in a vacuum its activity remains as great as at first. When enclosed in glass the glass soon assumes a pink colour, which is not superficial, as already observed by M. and Mme. Curie. By immersing a section of the glass in a liquid of the same refracting power as itself, the colour is seen to penetrate below the surface. If, however, the radium compound is sealed *in vacuo* in a quartz tube no coloration takes place, and I can detect no diminution of energy even in 12 months.

Electrons from radium will pass through aluminium and a considerable length of air and affect a sensitive film.† Experiments on this point were tried with polonium, and it was found that air offered great obstruction.

* 'Roy. Soc. Proc.' vol. 66, p. 421, May, 1900.

† Using an active compound of radium, I have obtained an impression on a sensitive film through a penny-piece.

Three shallow cells 17 mm. in diameter and 2 mm. deep were filled with polonium subnitrate, the same quantity in each cell, the surfaces being levelled. Over these cells were fixed three pieces of lead pipe of lengths of 1, 2, and 4 inches, so that each cell of polonium had its emanations confined to its own tube. Sensitive films were put over each tube, and the whole kept in total darkness for 144 hours. At the end of that stretch of time the films were developed. No image was seen on the film 4 inches from the polonium, a faint image was perceptible on the film 2 inches off, and a stronger one on the film 1 inch off. Measured in the opacity meter the figures were:—

Over 1 inch tube—Opacity log. = 0·18; Opacity = 1·51.

Over 2 inch tube—Opacity log. = 0·04; Opacity = 0·11.

A repetition of this experiment, using tubes of glass instead of lead, gave almost identical results.

The electron theory explains a fact which has long puzzled experimentalists. It is well known that if a coin is laid on a sensitive plate in perfect darkness and connected with one pole of an induction coil for a few seconds and then developed, an image can be obtained of the raised parts of the coin. This has generally been explained by saying that the electrified stream of air, or the “brush discharge,” affects the film like light.

But Mr. F. Sanford* shows that coins embedded in the centre of a block of paraffin 2 cm. thick, where they could not send off streams of electrified air, can still be photographed by means of the induction coil. Under these circumstances it is probable that electrons are the agents, as electrons will easily pass through paraffin wax from the coin to the sensitive plate, when the coin is connected with the negative pole of an induction coil, the other pole being connected with a metal plate placed below the wax block.

Hitherto we have been dealing with negative electrons—a free positive electron at present is unknown. In a paper communicated to the Royal Society, December, 1900,† the Hon. R. J. Strutt offers a suggestion as to positive ions which in a satisfactory manner appears to explain much that hitherto has been left doubtful, not to say contradictory.

He adopts the generally recognised theory that the deflectable Becquerel rays consist of a stream of negative corpuscles with enormous velocities proceeding from the radio-active body. But there are two kinds of Becquerel rays, one deflectable and penetrating, the other non-deflectable and easily absorbable. Mr. Strutt considers that these non-deflectable rays are positive ions moving in a stream from the radio-active body.

* ‘Nature,’ vol. 55, p. 485.

† ‘Phil. Trans.,’ A, 1901, vol. 196, p. 525.

He says:—"We know that the positive ions in gases carry the same charge as the negative, and that they have an enormously greater mass. Unless, therefore, their velocity is smaller out of all proportion than the negative ions, it is to be expected that they will be much less easily deflected by the magnet. . . . Next it may be noticed that the smaller penetrating power would be well accounted for by the size of the positive ions, which would, of course, make more collisions with the molecules of the surrounding gas than the much smaller negative ions."

Of the three radio-active bodies, radium, actinium, and polonium, actinium appears to emit corpuscles almost entirely of the penetrating, deflectable kind, polonium rays of the non-deflectable, non-penetrating kind, whilst radium emits rays of both kinds.

On the above hypothesis corpuscles from polonium might consist of the heavy positive ions: to test the accuracy of this inference experiments are now in progress.

Some curious and far-reaching inferences may be drawn from Mr. Strutt's view, supposing it to be correct, that positive as well as negative corpuscles will fly off from a radio-active body. In a paper "On Electrical Evaporation"* I showed that many bodies, such as silver, gold, platinum, &c., usually considered non-volatile at ordinary temperatures, easily volatilise in a vacuum if connected with the negative pole of an induction coil, remaining fixed when connected with the positive pole. This phenomenon was first observed by Dr. Wright, of Yale College, and was applied by him for the production of mirrors for physical apparatus. It is shown by experiments that the action in the vacuum tube is of two kinds. A silver pole was used, and near it, in front, was a sheet of mica with a hole in its centre. The vacuum was very high ($P = 0.00068$ mm.), and when the poles were connected with the coil, the silver being negative, electrons shot from it in all directions, and passing through the hole in the mica screen, formed a bright phosphorescent patch on the opposite side of the bulb. The action of the coil was continued for some hours, to volatilise a certain portion of the silver. On subsequent examination it was found that silver had been deposited only on the mica screen and in the immediate neighbourhood of the pole; the far end of the bulb, at the spot which had been glowing for hours from the impact of electrons, being free from silver deposit. Here then are two simultaneous actions. Electrons, or as I once called them "Radiant Matter," shot from the negative pole, and caused the glass against which they struck to glow with phosphorescent light. Simultaneously the heavy positive ions of silver, freed from their negative electrons, or under the

* 'Roy. Soc. Proc.,' vol. 50, p. 88, June, 1891.

influence of the electrical stress, likewise flew off, and were deposited in the metallic state near the pole.

An experiment was tried to discover if the ions of metal, deposited in this manner on a metal plate connected with an idle pole, in the full stream of + ions and - electrons, showed any special + or - electrification. In all cases the electrification was positive. This lends support to Mr. Strutt's view that + ions as well as - electrons will fly off from a radio-active body. Even these results, however, must not be taken as conclusive, for in a paper published in 1891,* I showed that at a high vacuum nearly the whole of the interior of a tube through which an induction spark was passing was electrified positively, negative electrification being detected only in the immediate neighbourhood of the negative pole.

During the course of my experiments a curious circumstance was observed, which deserves record as it may elucidate some of these obscure phenomena. While the volatilisation of the silver pole is rapidly proceeding, the metal glows as if red-hot. This "red heat" is superficial only. The metal instantly assumes, or loses, the appearance of red heat the moment the current is turned on or off, showing that the high temperature does not penetrate below the surface. The volatilisation of the positive ions is confined to the surface, and the surface glow is connected with that action. If instead of silver, a good conductor of heat, I take diamond, a bad conductor, the surface layers are changed sufficiently to convert them into a form of graphite, which from its great resistance to oxidising agents, cannot be formed at a lower temperature than 3600° C.

"The Density and Coefficient of Cubical Expansion of Ice." By
J. H. VINCENT, D.Sc., B.A., St. John's College, Cambridge.
Communicated by Professor J. J. THOMSON, F.R.S. Received
January 22,—Read February 6, 1902.

(Abstract.)

After an account of the methods employed by previous experimenters in the subject, reference is made to the views of Nichols, according to which two distinct kinds of ice have been subjected to experiments. The density of artificial ice is about 0.916 gramme per cubic centimetre, while that of natural ice is more than one part in a thousand greater.

* "Electricity in Transitu: from Plenum to Vacuum," 'Journ. Inst. Elect. Engin.,' vol. 20, p. 10.

Two tables are given showing in the first all the results which have been published concerning the density and coefficient of cubical expansion of ice, and in the second the same results tabulated separately according to the variety of ice used. The mean result for the density of natural ice at freezing point is 0.9176, while that of artificial ice is 0.9162 gramme per cubic centimetre. Only one estimation of the dilatation of natural ice is available. It is 0.0001125 for the cubical coefficient per degree C. The mean of three available results for artificial ice is 0.000160.

It was thought desirable to use a method of experiment which would yield a result both for the density and for the coefficient of cubical expansion, and in order that the work should have any value it was necessary to employ some device other than any which had been used previously.

The method consisted in weighing a quantity of water in mercury. The water was weighed both as liquid at 0° C., and as solid at several temperatures below freezing point. If we assume values for the density of water and mercury at 0° C., the density of ice at 0° C. can then be calculated, if we also assume that the densities of ice and mercury are linear functions of the temperature. The coefficient of cubical expansion of ice can also be calculated from these results, but it will depend on the law assumed for the contraction of mercury, and upon the accuracy of the thermometry.

Instead of using a sinker to keep the vessel containing the water or ice under the surface of the mercury, a modification of Joly's Hydrostatic Balance was employed.*

Ten values of the density of ice at different temperatures below 0° C. were obtained in this way. The specimens of water were four in number, and the temperatures ranged from -10°.02 to -0°.37 C. The whole of the weighings taken with the final form of apparatus are included in these determinations.

In one experiment the values obtained show unmistakably that the same specimen of water may assume different densities on freezing.

The ten values of the density are set out as functions of the temperature on a chart, and a graphical method is used to extrapolate for five values of the density of 0° C. These five values have weights assigned to them proportional to the number of separate determinations of the density from which they are derived. The numbers thus obtained and the weights to be assigned to them are set out in the following table.

* Joly, 'Phil. Mag.,' September, 1888.

424 *The Density and Coefficient of Cubical Expansion of Ice.*

Experiment.	Density at 0° C.	Weight assigned.
1	0·916335	3
2	0·915460	2
3	0·916180	2
4	{ 0·915540	1
	{ 0·916060	2
Weighted mean...	0·9160	

We thus obtain 0·9160 gramme per cubic centimetre as the density of ice at 0° C.

Four values for the coefficient of cubical expansion can be obtained from these results. They are :—

Experiment.	Coefficient of cubical expansion.
1	0·000155
2	0·000152
3	0·000153
4	0·000148
Mean	0·000152

The results of this investigation are that Nichols' value for the density of artificial ice is confirmed, but that since the same specimen of water can freeze into specimens of ice having different density, the use of the Bunsen ice calorimeter in absolute determinations must be limited to an accuracy of probably one in a thousand.

The expenses of this research have been in part defrayed by a Government grant from the Royal Society, and in part by the Cavendish Laboratory.

I wish to thank Professor J. J. Thomson, F.R.S., for his kind encouragement, and my thanks are also due to Mr. E. H. Griffiths, F.R.S., through whom I was led to undertake the investigation.

February 13, 1902.

Sir WILLIAM HUGGINS, K.C.B., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

- I. "On the Sub-Mechanics of the Universe." By Professor OSBORNE REYNOLDS, F.R.S.
- II. "On Chemical Statics and Dynamics under the Action of Light." By Dr. MEYER WILDERMAN. Communicated by Dr. LUDWIG MOND, F.R.S.
- III. "Preliminary Note on a Method of Calculating Solubilities, Equilibrium Constants of Chemical Reactions, and Latent Heats of Vaporisation." By Dr. A. FINDLAY. Communicated by Professor RAMSAY, F.R.S.
- IV. "The Refractive Indices of Fluorite, Quartz, and Calcite." By J. W. GIFFORD. Communicated by Professor S. P. THOMPSON, F.R.S.

"On the Sub-Mechanics of the Universe." By OSBORNE REYNOLDS, F.R.S. Received February 3,—Read February 13, 1902.

(Abstract.)

Sections.

- I. Introduction—Sketch of the results obtained and the steps secured.
- II. The general equations of motion of any entity.
- III. The general equations of motion in a purely mechanical medium, for mass, momentum, and energy.
- IV. The equations of continuity for component systems of motion.
- V. The mean and relative motions of a medium.
- VI. The approximate equations of component systems of mean- and relative-motion.
- VII. The general condition for the continuance of component systems of mean- and relative-motion.
- VIII. The conducting properties of the absolutely rigid granule-ultimate atom or primordium.
- IX. The probable ultimate distribution of the members of granular media, as the result of encounters, when there is no mean motion.
- X. Extensions of the kinetic theory to include rates of conduction of momentum and energy through the grains, when the medium is in ultimate condition and under no mean strains.
- XI. The redistribution of angular inequalities in the relative system.

- XII. The linear dispersion of mass, and of momentum and energy of relative motion by convection.
- XIII. The exchanges between the mean and relative systems.
- XIV. The conservation of inequalities in the mean mass and their motions about local centres.
- XV. The determination (1) of the relative quantities α'' , λ'' , σ , and G which define the state of the medium by the results of experience; (2) the general integration of the equation.

1. In this paper it is shown that there is one, and only one, conceivable purely mechanical system capable of accounting for all the physical evidence, as we know it, in the universe.

The system is neither more nor less than an arrangement of indefinite extent of uniform spherical grains, generally in normal piling, so close that the grains cannot change their neighbours, although continually in relative motion with each other, the grains being of changeless shape and size, thus constituting, to a first approximation, an elastic medium, with six axes of elasticity symmetrically placed.

The diameter of a grain, in C.G.S. units, is

$$5.534 \times 10^{-18} = \sigma.$$

The mean relative velocities of the grains are

$$6.777 \times 10 = \alpha''.$$

The mean path of the grains

$$8.612 \times 10^{-28} = \lambda''.$$

These three quantities completely define the state of the medium in spaces where the piling is normal; they also define the mean density of the medium as compared with the density of water as

$$10^4 = 22\Omega.$$

The mean pressure in the medium equal in all directions is

$$1.172 \times 10^{14} = p.$$

The coefficient of the transverse elasticity resulting from the gearing of the grains where the piling is normal is

$$9.030 \times 10^{20} = n.$$

The rate of propagation of the transverse wave is

$$3.000 \times 10^{10} = \tau \cdot (n \cdot p).$$

The rate of propagation of the normal wave is

$$7.161 \times 10^{10} = 2.387 \times \tau.$$

The rate of degradation of the transverse waves, *i.e.*, the dissipation resulting from the angular redistribution of the energy or viscosity is

$$5.603 \times 10^{-16} = t_t,$$

or such as would require 56,000,000 years to reduce the total energy in the wave in the ratio $1/e^2$, or to one-eighth, thus accounting, by mechanical considerations, for the blackness of the sky on a clear dark night, while the degradation of the normal wave, *i.e.*, the dissipation resulting from the linear redistribution of energy is such that the initial energy would be reduced to one-eighth in the 3.923×10^{-6} th part of a second, or before it had traversed 2200 metres; and thus would account, by mechanical considerations, for the absence of any physical evidence of normal waves, except such evidence as might be obtained within some thousand metres of the origin of the waves, as in the case of Röntgen rays.

2. In spaces in which there are local unequal inequalities in the medium about local centres owing to the absence or presence of a number of grains in deficiency or excess of the number necessary to render the piling normal, such local inequalities are permanent, and are attended with inward or outward strains, as the case may be, extending indefinitely throughout the medium, causing dilatation equal everywhere to the strains, but of opposite sign, *i.e.*, dilatation equal to the volume of the grains, the presence or absence of which causes the inequality.

When the arrangement of the grains about the centres is that of a nucleus of grains in normal piling, on which the grains in the strained normal piling rest, the nucleus in normal piling cannot gear with the grains outside in strained normal piling; so that there is a singular surface of misfit between the nucleus and the grains in strained normal piling. Such singular surfaces are surfaces of weakness, and may be surfaces of freedom, or surfaces of limited stability with the neighbouring grains.

These singular surfaces, when their limited stability is overcome, are free to maintain their motion through the medium, by a process of propagation in any direction, the number of grains entering the surface on the one side being exactly the same as the number leaving on the other side, so that when the inequalities are the result of the absence of grains, they correspond to the molecules of matter.

If the singular surface of a negative inequality is propagating through a medium which is at rest, the grains forming the nucleus will have no motion, whatever may be the motion of the singular surface. But the strained normal piling which surrounds the singular surface and moves by propagation with the singular surface, being of less density than the mean density of the medium, represents a displacement of the negative mass of the inequality, *i.e.*, of the grains absent,

and in whatever new direction the singular surface is propagated, the motion of the medium outside is such as represents equal and opposite momentum, as when a bubble is rising in a glass of water.

In exactly the same way, for inequalities resulting from an excess of grains, the momentum resulting from the displacement of the medium would be positive.

The principal stresses in the medium outside the singular surface of a negative inequality are to a first approximation two equal tangential pressures equal in all directions :—

$$p_t = \frac{6}{5} p;$$

and a normal pressure :—

$$p_n = \frac{3}{5} p;$$

the mean of these pressures being everywhere the mean pressure of the medium equal in all directions.

Efforts, proportional to the inverse square of the distance, to cause two negative inequalities to approach from a finite distance, are the result of those components of the dilatation (taken to a first approximation) which are caused by the variation of those components of the inward strains which cause curvature in the normal piling of the medium.

The other components of the strain, being parallel distortions, which satisfy the condition of geometrical similarity, do not affect the efforts. If the grains were indefinitely small there would be no effort; thus the diameter of a grain is the parameter of the effort, and, multiplying this diameter by the curvature of the medium and again by the mean pressure of the medium, the product measures the intensity of the effort.

The dilatation diminishes as the centres of the negative inequalities approach, and work is done by the pressure in the medium outside the singular surfaces to bring the negative inequalities together.

The efforts to cause the negative inequalities to approach correspond exactly to the gravitation, if matter represents negative mass.

Taking the mean density of the earth as -5.67 as compared with water (-1),

The reciprocal of the density of the medium being	0.10^{-4} ,
The mean pressure of the medium	1.172×10^{14} ,
or the diameter of a grain	5.534×10^{-18} ,
The mean radius of the earth	6.3709×10^8 ,

the effort to cause approach between the earth and a unit of matter on the earth's surface (-1) is the product of these quantities multiplied by $4\pi/3$, or:

$$p\sigma \times 10^{-4} \times \frac{4}{3} \pi \times 5.67 \times 6.3709 \times 10^8 = 9.81 \times 10^2.$$

The inversion is thus complete; matter is the absence of mass, and the effort to bring the negative inequalities together is also an effort on the mass to recede. And since the actions are those of positive pressure there is no attraction involved; the efforts being the result of the virtual diminution of the pressure inwards.

3. If instead of the negative inequalities as in the last article the inequalities are positive the efforts are reversed, tending to separate the inequalities, and the analysis would be the same, except that the curvature would be negative. And it is important to notice that if such positive inequalities exist the fact that they repel each other, *i.e.*, that they would tend to scatter through space, together with the evidence that the number of inequalities, either positive or negative, occupy an indefinitely small space as compared with the total volume of the medium, places any importance such positive inequalities might have on a footing of indefinitely less importance than that of the negative inequalities which are caused to accumulate by gravitation; and thus we have an explanation of any lack of evidence of any positive inequalities—even if such exist.

4. Besides the positive and negative inequalities, there is another inequality which may easily be conceived—and that is of fundamental importance. Whatever may be the cause it is possible to conceive that a number of grains may be removed from one position in the medium to another, the medium being otherwise uniform; thus instituting a complex inequality as between two inequalities, one positive and the other negative; the number of grains in excess in the one being exactly the same as the number of grains absent in the other.

The complex inequalities differ fundamentally from the gravitating inequalities, inasmuch as the former involve an absolute displacement of mass, while the latter have no effect on the mean position of the mass in the medium, and in respect of involving absolute displacement of mass the complex inequality corresponds with electricity.

Apart from the displacement of mass, the complex inequalities differ from the gravitating inequalities. In the complex inequalities the parameter of the dilatation is not the diameter of a grain, but one-half the linear dimension of the volume occupied by the grains displaced, taken as spherical.

The effort to revert in the case of the complex inequality is the product of the pressure multiplied by the product of the volume of the positive and negative inequalities, and again by the parameter, r_0 . This is expressed when the positive and negative inequalities are at finite distances apart by

$$p \left(\frac{4\pi}{3} \right) r_0^3 = -R,$$

R being essentially negative, and the dimensions of the effort ($-R$) are mlt^{-2} , which express an effort to the displacement of mass.

The complex inequality which corresponds to the separation of the positive and negative inequalities is one displacement, not two. This fact admits of no question, and might have been recognised long ago had it not been for the general assumption that positive electricity repels positive electricity, the fact being that the apparent repulsion of the positive electricities is the result of their respective efforts to approach their respective negative inequalities. By the assumption it became apparently possible to express the potential v and the electricity q as rational quantities, while, as it now appears, the potential r and the electricity q are respectively

$$-(e^2)^{\frac{1}{4}} \cdot \frac{1}{r} \quad \text{and} \quad (-e^2)^{\frac{1}{4}},$$

which are both irrational. Their product being the rational quantity

$$e^2/r,$$

which differentiated with respect to the distance is

$$-e^2/r^2 = R.$$

And the mechanical explanation of these is

$$-\left[-p\left(\frac{4\pi}{3}\right)^2 \cdot r_0^7\right]^{\frac{1}{4}} \cdot \frac{1}{r} = v, \quad \left[-p\left(\frac{4\pi}{3}\right)^2 \cdot r_0^7\right]^{\frac{1}{4}} = q,$$

and for the effort to revert we have—

$$p\left(\frac{4\pi}{3}\right)^2 \cdot \frac{r_0^7}{r^2} = -R.$$

Then for the electrostatic units we have, since $r = 1$, and $R = -1$,

$$p\left(\frac{4\pi}{3}\right)^2 r_0^7 = 1,$$

and from the known value of p , the number of grains displaced through unit distance necessary to cause the unit effort is

$$1.615 \times 10^{15},$$

and $r_0 = 6.493 \times 10^{-3}$, from which we have the ratio of the effort to reinstate the normal piling, to the effort of gravitation, from the same number of grains absent in each inequality as are displaced in the complex inequality, the distances being the same

$$1.2 \times 10^{15};$$

so that the effort of attraction between two inequalities, the grains absent about each of which are the same as the grains displaced in

instituting the complex inequality, is eighty-one thousand billions less than that of the electric effort.

5. Cohesion between the singular surfaces of the negative inequalities results from the terms which were not taken into account in the first approximation, which corresponds to gravitation. These secondary terms involve the inverse distance to the sixth power, and have therefore a very short range, and so correspond to efforts of cohesion of the singular surfaces as well as surface tensions, having no effect when the singular surfaces, or molecules, are separated by a distance very small compared with the diameter of the singular surface.

6. Transverse undulations in the medium corresponding to the waves of light are instituted by the disruptive reversion of the complex inequalities. The recoil sets up a vibration which is exhausted in initiating light.

7. Thus far the sketch of the results has included only those for which there exists sufficient evidence to admit of definite quantitative analysis. Nevertheless, these quantitative results show that the granular medium, as already defined, accounts by purely mechanical considerations for the evidence, and affords the only purely mechanical explanation possible. If then the substructure of the universe is mechanical, all the evidence not already adduced is such as may be accounted for by an extension of the analysis. And this is found to be the case.

The results of the further analysis afford proof—

Of the existence of coincidence between the periods of vibration of the molecules and the periods of the waves ;

That dissociation of the compound molecules proves the previous state to have been one of limited stability ;

That the reassociations of compound molecules result from the reversion of complex inequalities ;

Of the absorption of the energy of light by inequalities ;

That negative inequalities affect the waves passing through ;

That refraction is caused by the vibration of inequalities having the same periods as the waves ;

That dispersion results from the greater number of coincidences as the waves get shorter ;

That the polarisation by reflection is caused only by that component of the transverse motion in the medium which is in the plane of incidence and results from the passage of the light from a space without, or with few, inequalities, through a surface into a space in which there are more inequalities ;

That the metallic reflection results from the relative smallness of the dimensions of the molecules compared with the length of the wave and the closeness of their piling when the waves pass from

a space without inequalities across the surface beyond which the inequalities are in closest order ;

That the aberration of light results from the absence of any appreciable resistance to the motion of the medium when passing through matter.

8. It may be somewhat out of the usual course to describe the results of a research before any account has been given of the method by which these results have been obtained ; but in this case the foregoing sketch of the purely mechanical explanation of the physical evidence in the universe by the granular medium has seemed the only introduction possible. And even so it is not with any idea that this introduction can afford any preliminary insight as to the methods by which these results have been obtained.

Certain steps, as it now appears, were taken for objects quite apart from any idea that they would be steps towards the mechanical solution of the problem of the universe. The first of these steps was taken with the object of finding a mechanical explanation of the sudden change in the rate of flow of the gas in the tubes of a boiler when the velocity reached a certain limit ; perhaps this would be better described as a step towards a step.*

The second step was the discovery of the thermal transpiration of gas together with the analytical proof of the dimensional properties of matter.†

The third step was the discovery of the criterion of the two manners of motion of fluids,‡ and it was only on taking the fourth step, namely, the study of the action of sand, which revealed dilatancy as the ruling property of all granular media§ which directed attention to the possibility of a mechanical explanation of gravitation. In spite of the apparent possibility all attempts to effect the necessary analysis failed at the time.

There was, however, a fifth step : the effecting of the analysis for viscous fluids, and the determination of the criterion,|| which led to the recognition of the possibility of the analytical separation of the general motion of a fluid into mean varying motion, displacing momentum, and relative motion without mean momentum ; and this suggested the possibility that the medium of space might be granular, the grains being in relative motion, and at the same time being subject to varying mean motion. And this has proved to be the case.

At the same time it became evident that it was not to be attacked by any method short of the general equations of a conservative system

* 'Manchester Lit. and Phil. Soc.,' 1874-5, p. 7.

† 'Phil. Trans.,' 1879.

‡ 'Phil. Trans.,' 1883.

§ 'Phil. Mag.,' 1885.

|| 'Phil. Trans.,' 1895.

starting from the very first principles; and it is from such study that this purely mechanical account of the physical evidence has been obtained.

“Micro-crystalline Structure of Platinum.” By THOMAS ANDREWS,
F.R.S., F.C.S. Received October 26,—Read November 28,
1901.

[PLATE 6.]

The crystalline structure of platinum does not appear to have been studied, although it forms an interesting subject for investigation.

A small ingot of pure platinum was obtained for the experiments. A section was cut therefrom and machined to $\frac{5}{16}$ inch square and $\frac{1}{16}$ inch in thickness. The section was then carefully polished and etched in aqua regia of the following strength:—

4 parts of hydrochloric acid (sp. gr. 1.2).
1 part of nitric acid (sp. gr. 1.42).

When the polished micro-section was immersed in the aqua-regia solution in the cold, no solvent action occurred, although the metal was exposed for a considerable time in the reagent.

The temperature of the etching solution was then gradually raised to boiling point, at which it was maintained for 15 seconds, but it had apparently no visible effect on the platinum, as ascertained by microscopical examination, after having previously washed the section with water and pure alcohol.

The etching process was repeated and the metal again exposed for a further period of 30 seconds, but after microscopical examination of the surface of the metal it was found that even this exposure had not satisfactorily developed a structure in the metal.

The etching process was again repeated and the micro-section exposed for another 15 seconds to the boiling aqua-regia solution above described. This developed a better crystalline structure, though not a thoroughly satisfactory one. The same micro-section of platinum was therefore again exposed to the action of boiling aqua regia for a further period of 45 seconds, resulting in the development of a beautiful crystalline structure which manifested not only the large or primary crystal grains but also the secondary or very minute crystalline development which is illustrated on Plate 6.

Fig. 1 shows the micro-crystalline structure of pure platinum as seen in transverse section by oblique illumination at a magnification of 50 diameters.

Fig. 2 shows, at a magnification of 120 diameters, the crystalline structure as indicated in transverse section by the direct or vertical method of illumination. This micro-section illustrates the general

form of the primary or large crystal grains, and also indicates the existence of very minute crystals within the boundary lines of the larger crystal grains.

The structure and general orientation of the minute secondary crystals, as seen in section by vertical illumination at the higher magnification of 360 diameters, are illustrated on figs. 3 and 4.

The larger or primary crystal grains were observed to consist of irregular polygons of varying size, the etched indications of the facet junctions being often clearly and sharply defined.

The minute or secondary crystals (whose inter-crystalline junctions were also clearly seen) were in the mass observed to be in varied positions of orientation, but the orientation was generally identical, or on the same plane, within the area of each larger crystal grain.

The general orientation of the smaller crystals varied, however, in each separate larger crystal, and the consequent varied reflection of the light has given the face of the micro-section, as a whole, the appearance of lighter or darker areas in the photo-micrographs. This effect is merely optical.

In some portions of the mass there were observed minute triangular crystals; these appear, however, to be only developments resulting from the cutting of certain crystals in section.

The general micro-crystalline structure of platinum was observed to be allotriomorphic in character and derived from a system of interfering cubes and octahedra, the cubic and hexagonal form being frequently noticeable.

The size of the large crystal grains varied from about 0.002 inch to 0.04 inch in size, and the smaller crystals ranged from about 0.0002 inch to about 0.007 inch.

There were indications that the smaller or secondary crystals were each built up of even more minute crystalline ramifications.

The crystalline structure of platinum appears to generally resemble that of gold and silver.

The descriptive words "primary" and "secondary" crystals are only used in relation to size, and they are not intended to convey the idea of distinctive times of formation during solidification.

Addendum. February 3, 1902.

In connection with the present investigation reference may be made to the valuable researches of Professor Liversidge on "The Crystalline Structure of Gold and Platinum Nuggets."* On comparison there appears to be considerable identity between the crystalline structure of natural platinum nuggets and that of the platinum ingot

* 'Journal, Royal Society of New South Wales,' 1897, vol. 31, p. 70, Plate 9.

108.

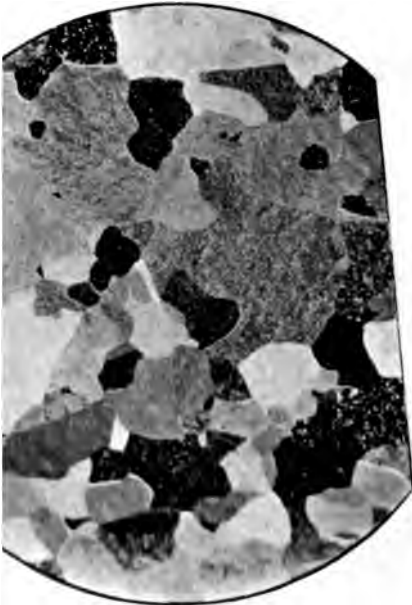


FIG. 1. $\times 50$ diams.

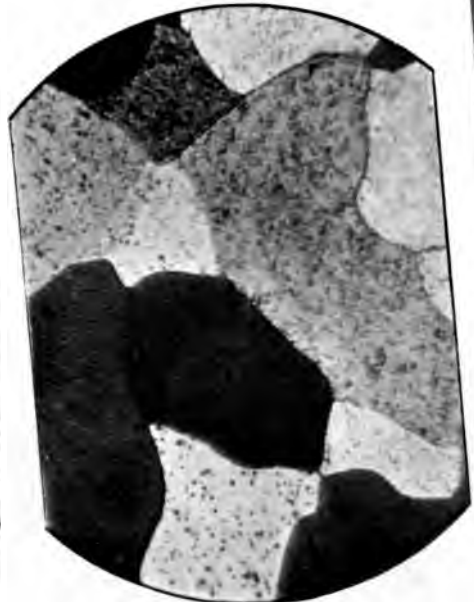


FIG. 2. $\times 120$ diams.

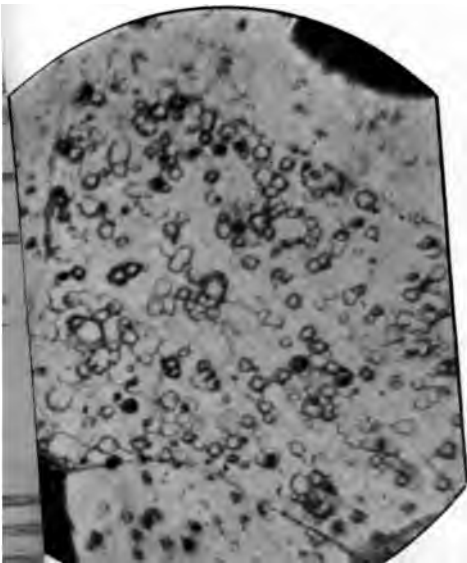


FIG. 3. $\times 300$ diams.

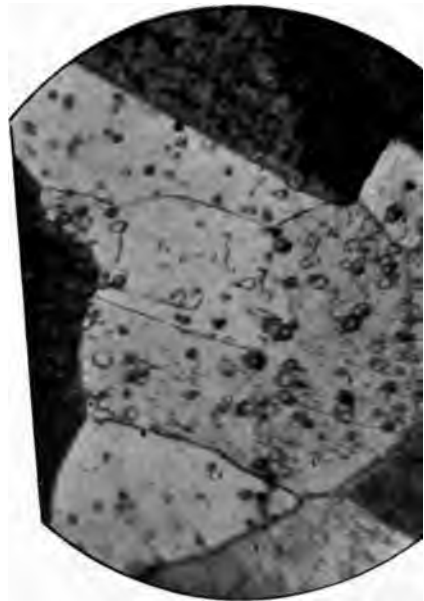


FIG. 4. $\times 360$ diams.



;

examined by the author (Mr. Thos. Andrews), the crystalline jointings appearing to be similar in character.

A general analogy appears also to exist between the crystalline structure of pure platinum and that of pure gold, which is noticeable on comparing the illustrations in the present paper with those in the following papers, viz.: "On the Structure of Gold and Gold Alloys," by Professor J. O. Arnold,* and "On the Structure of Metals, its Origin and Changes," by M. Osmond and Sir Roberts-Austen.†

This identity of structure is further seen on referring to the illustrations in the paper on "The Microscopic Structure of Gold and Gold Alloys," by Mr. Thomas Andrews.‡

DESCRIPTION OF PLATE 6.

Micro-crystalline Structure of Platinum.

- Fig. 1. Structure seen in section magnified 50 diameters. Oblique illumination.
Fig. 2. Primary and secondary crystals seen in section magnified 120 diameters. Vertical illumination.
Fig. 3. Primary and secondary crystals seen in section magnified 360 diameters. Vertical illumination.
Fig. 4. Primary and secondary crystals, magnified 360 diameters. Vertical illumination.
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"The Distribution of Magnetism as Affected by Induced Currents in an Iron Cylinder when Rotated in a Magnetic Field." By ERNEST WILSON, Professor of Electrical Engineering, King's College, London. Communicated by Sir W. H. PREECE, F.R.S. Received January 3,—Read January 30, 1902.

One object of this research§ is to investigate the effect which induced currents have upon the distribution of magnetism in an iron cylinder when it is rotated about its longitudinal axis in a magnetic field whose direction is normally at right angles to the axis of rotation. Another object is to investigate the rate of dissipation of energy by these induced currents, and to compare the same with the result of theory. This second part of the work will be dealt with in a subsequent paper.

The variables dealt with are the total flux of magnetism between

* 'Engineering,' vol. 61, 1896, p. 176.

† 'Phil. Trans.,' A, 1896, p. 417, fig. 10, Plate 9, and fig. 16, Plate 10.

‡ 'Engineering,' September 30, October 28, December 9, 1898; see also "Micro-metallography of Iron," by Mr. Thomas Andrews, 'Roy. Soc. Proc.,' vol. 58, 1895.

§ I wish to acknowledge the grant of £80 for the purposes of this research, which was voted to me by the Council of the Royal Society out of the Government Grant.

the poles of the magnet used and the speed of rotation of the cylinder. The cylinder is of mild steel, and has diameter and length each 10 inches (25.4 cms.). It is shown in section in fig. 1. In order to find the average value of the intensity of induction over elements of the cross-section of the cylinder, holes $\frac{1}{4}$ inch (0.635 cm.) diameter are drilled as shown in a plane containing the axis of the cylinder. By threading insulated copper wires through these holes certain areas were surrounded and the ends of the coils brought out through a hole in the gunmetal shaft to a terminal board fixed thereto. The areas in fig. 1, numbered 1, 2, 3, I, II, III, have in each case been surrounded by exploring coils, each coil consisting of nineteen complete turns. Another coil is wound entirely round the cylinder in the same plane of section and numbered 4. A D'Arsonval galvanometer and resistance box were included in each circuit, and twin wires were used in each case to reach from the terminal board to the galvanometers.

The magnet consists of two slabs of soft iron each $40\frac{1}{2}$ inches (103 cms.) long, 19 inches (48.3 cms.) broad, and $8\frac{1}{2}$ inches (21.6 cms.) thick, joined together by a yoke at one end, giving a distance of 8 inches (20.3 cms.) between the opposing broad sides of the slabs. The cylinder rotates in gunmetal bearings bolted to the poles of the magnet, which are tapered so as to concentrate a powerful magnetic field upon the cylinder if necessary. The bore of the pole-pieces is $10\frac{1}{4}$ inches (25.7 cms.) diameter. The arc embraced by each pole-piece is 170° , and the length of the pole-piece next to the cylinder is 10 inches. The cylinder is turned by means of a worm and worm wheel. The worm wheel has 90 teeth, and the worm a single thread, so that one revolution of the worm shaft per second corresponds to a periodic time of 90 seconds for the cylinder. The wheel on the worm shaft is turned by hand, while by aid of a clock beating seconds and a scale fixed under the wheel the speed is controlled. For the highest speed the ratio of the gearing was increased four-fold. The operator at the wheel counted seconds aloud, thus enabling the epoch of the simultaneous observations at the galvanometers to be determined. The electromotive forces so obtained have been plotted in terms of the time on squared paper, and by integration the average value of the induction density with respect to any coil has been found. The area taken for each coil is that defined by the centre line of the drilled holes. The areas of coils Nos. 1, 2, 3 are each taken to be 25.8 sq. cms. The areas of coils I, II, III are taken to be 49.2, 51.6, 51.6 sq. cms. respectively, and the area of No. 4 coil is taken to be 645 sq. cms. Throughout the paper the curves of electromotive force are numbered to agree with the coils from which they have been obtained.

Before dealing with the results obtained, it may be stated that

these experiments have a wider application than to the cylinder experimented upon. Consider two similar homogeneous cylinders having the same magnetic properties and the same specific resistance, and let the ratio of their dimensions be n . As the magnetism penetrates from the outside into the iron by a diffusion process, and a kinematic coefficient of diffusion is of two dimensions in space and one inversely in time, it follows that if the cylinders be each rotated in a magnetic field of the same intensity, then to induce the same electromotive force at similar points the speed of rotation must vary inversely as n^2 , and the induced currents will be similar in the two cylinders. In Table I is given the periodic time or frequency at which similar electric and magnetic events will happen in similar cylinders of different diameters.

I. *Radial Variation of Intensity of Magnetic Induction.*—Five different periodic times have been dealt with, namely, 360, 180, 90, 45, and 22.5 seconds. The collected results for 360, 90, and 45 seconds are given in figs. 2, 4, 6 respectively, and have been taken from Table II which contains the complete data. The curves show approximately how the maximum intensity of induction varies at different depths of the cylinder, and have been obtained by first plotting the maximum average induction per sq. cm. for each coil 1, 2, 3 at its mean radius. We notice at once how marked is the effect of variation of frequency upon the distribution of induction. Moreover, for the smaller periodic times we notice how much more marked are the effects for intermediate values of the external magnetising force.

The maximum intensity of induction at the surface of the cylinder has been taken from curves, and in each case the corresponding value at the centre has been expressed as a percentage of it. Fig. 7 gives the results so obtained plotted in terms of the maximum intensity of induction at the surface. The curves are numbered 360, 180, 90, 45, 22.5 to correspond with the periodic time in each case. We see that as the frequency is increased the centre is more shielded; moreover with increase of frequency the range over which the interior of the cylinder is shielded is increased. At 45 seconds periodic time the surface value of the intensity of magnetic induction at which the effects of induced currents begin to rapidly diminish is about 16,000. At 90 seconds it is about 11,000, but the rate is slower. At 180 seconds the change starts at about 7000 and is more gradual. At 360 seconds the disturbing effects due to induced currents are probably small. The distribution of intensity of induction when the cylinder is not rotating is not known, and probably varies with the intensity of magnetic force. The curves in fig. 2 may perhaps be trusted to give some indication of the distribution if the cylinder was not rotating. The dotted curve in fig. 2, giving B 15,400 at the centre of the cylinder, probably gives this value too low, owing to some unknown error. The curve is

given, as the author does not wish the paper to show a greater accuracy than the experiments are entitled to.

The way in which the magnetism is crowded to the surface at the higher speeds can be well seen in Table II. With a periodic time of 22.5 seconds the value of maximum average induction density near the surface is 10,770, for a maximum average over the whole cylinder of 6850. With a periodic time of 360 seconds a maximum average over the whole cylinder of 6970 is obtained with a maximum average near the surface of 7170. The values of maximum induction density at the surface are 17,000 and 7000 in the above examples respectively.

II. *Alternating Magnetic Force.*—It is interesting to compare the results just given with those obtained from a cylinder 12 inches (30.5 cms.) diameter when under the influence of an alternating magnetic force directed along the longitudinal axis. This cylinder is of mild steel and has probably nearly the same magnetic and electric properties as the cylinder in fig. 1 at the same temperature. Holes are drilled in the 12-inch cylinder in a plane at right angles to the longitudinal axis in much the same manner as in the rotating cylinder. Taking the areas as bounded by the centre lines of the drilled holes, the data in Table III have been obtained, and the curves in figs. 3, 5 have been plotted therefrom.* In fig. 3 the periodic time was 10.3 minutes, corresponding to a frequency of 150 periods per second with a cylinder 0.1 cm. diameter. In fig. 5, the periodic time was 2.6 minutes, corresponding to a frequency of 600 periods per second with a cylinder 0.1 cm. diameter. We see by Table I that figs. 2 and 4 can refer to a cylinder of diameter and length 0.1 cm., when rotating at frequencies 179 and 717 periods per second. Comparing figs. 4 and 5, we see that there is a similarity, but with the alternating magnetic force the shielding effect is more severe than with the rotating field.

This is perhaps better seen by referring to fig. 7, where the points marked $\times$ are taken from fig. 5, and should be compared with curve 90. It will be seen that with the alternating magnetic force, the severity is about as great as at 1434 periods per second (curve 45) with the rotating force. Similarly, by comparing figs. 2 and 3, and the results obtained therefrom in fig. 7, namely, curve 360 and the points marked $\times$, we see that for the same diameter of cylinder (0.1 cm.) the effects are much more marked with the alternating force. The points lie fairly well on the curve numbered 180. The same remarks refer to the maximum average over the whole section.

It should be noted that the area of No. 1 coil in these experiments, which is the same as the innermost coil in the case of the alternating

* See Wilson, 'Roy. Soc. Proc.,' vol. 68, p. 218. The figures in *italics* in the table at the end of this paper have been re-calculated, owing to an error in estimating the areas.

magnetic force, bears a somewhat larger ratio to that of the whole section (fig. 1) than in the case of the alternating force.

The conclusion is, however, that with an alternating magnetic force having a direction parallel to the longitudinal axis of a cylinder of given diameter, the effects of induced currents examined in this paper are more severe than in the same cylinder, of length equal to diameter, when rotated about its longitudinal axis in a magnetic force whose direction is at right angles to the axis of rotation for corresponding values of the frequency and the surface-induction density.

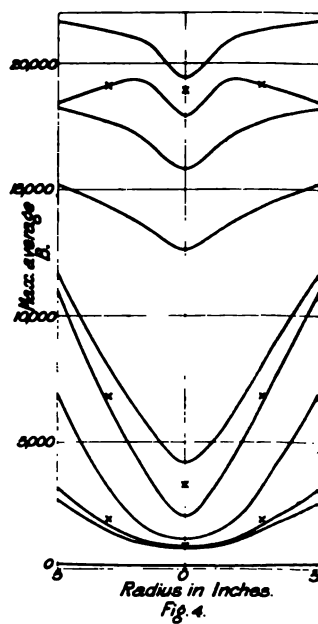
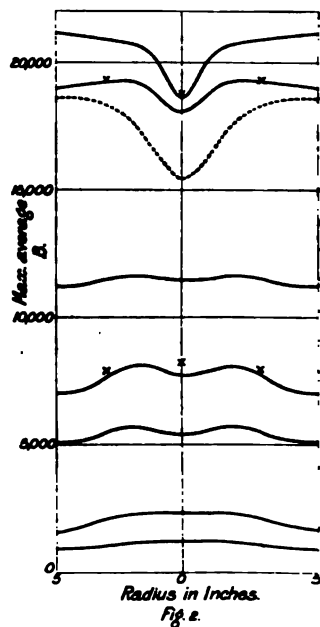
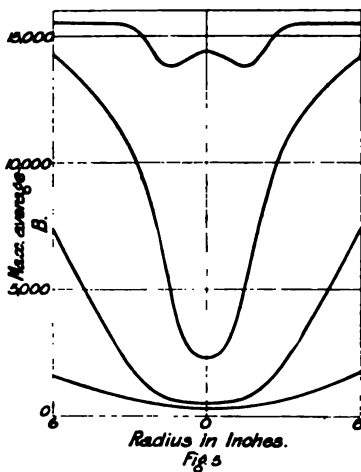
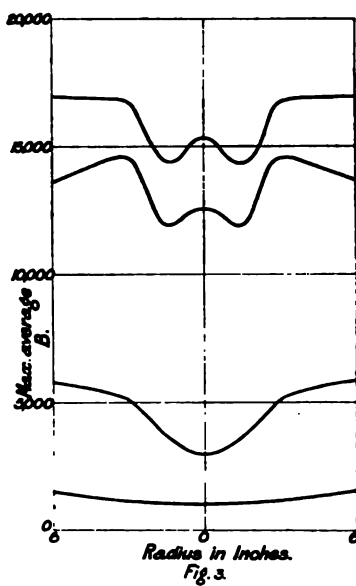
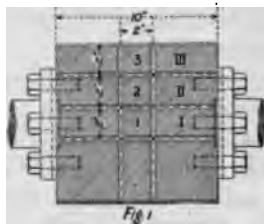
It should be remembered that in the case of alternating magnetic force, the electromotive force of a coil embedded in the iron is due to a true reversal of magnetism in the iron, and all the magnetic forces can be referred to a given cross-section of the iron cylinder. With a rotating field, the magnetic forces are referred to position in space.

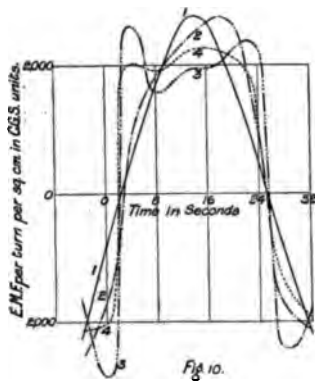
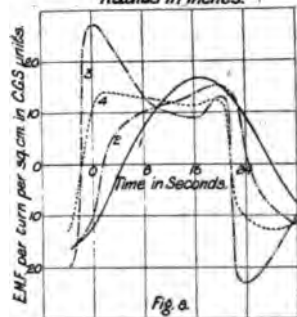
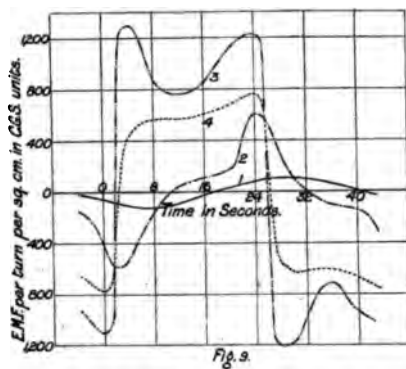
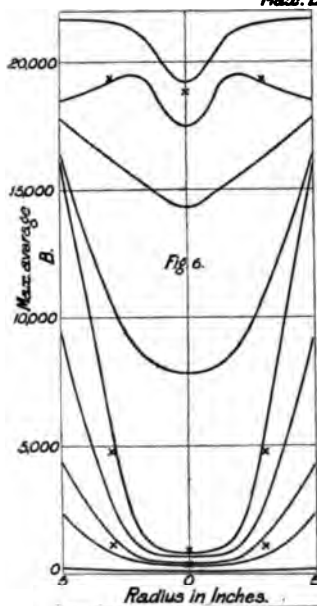
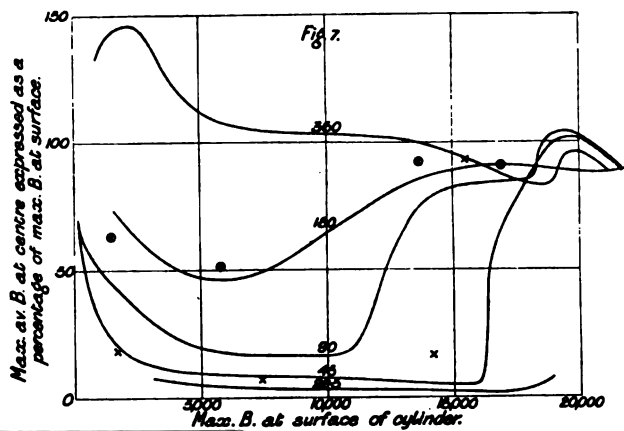
Table III gives the values of the intensity of magnetic induction at different points of the 12-inch cylinder under alternating magnetic force. It also gives the relative phase-displacement of the electromotive-force curves obtained from the exploring coils.

III. *Phase-displacement.*—The preceding remarks have dealt with the maximum intensity of induction experienced each half-period at different points along a radius of the cylinder. In Table II are given the phase-displacement of the electromotive-force curves for coils 1, 2, and 4 with regard to the electromotive force of No. 3 coil. Referring to fig. 6, we saw how very sensitive the induction density at the centre of the cylinder is to changes at the surface in the region of 16,000, and this is when the maximum phase-displacements are experienced. For a very small change in the intensity of magnetic induction at the surface, when of the order 16,000, the interior of the cylinder experiences variations ranging from 738 to 7830. The phase-displacements change very abruptly, for when the induction density at the surface is 18,500, the phase between the electromotive-forces of coils 1 and 3 has dropped to 15° from 122° at 16,400. For very high induction density the curves are practically in phase. With the smallest value of induction density at the surface (180), the phase-difference between the electromotive forces of Nos. 1 and 3 coils is 44° .

The phase-displacements in Table II have been judged from the points at which the respective electromotive-force curves cross the axis of time. This can be done with a good deal of certainty, but with the alternating magnetic force (Table III) this is not always the case. If reference be made to the electromotive-force curves,* it will be seen how different in character they may be when compared with those obtained with the rotating cylinder. In Table III the phases have been judged from the centre of gravity of the area inclosed by the electromotive-force curve and the axis of time per half-period.

* See Wilson, 'Roy. Soc. Proc.,' vol. 68, p. 218.





With alternating magnetic force the phase-displacement is more severe than in the corresponding condition with a rotating magnetic field. At frequency 600 with alternating magnetic force the maximum phase-displacement with a cylinder 0.1 cm. diameter is of the order 160° . With rotating magnetic force of frequency 1434, for a cylinder of diameter and length each 0.1 cm., the maximum phase-displacement is 122° and at 717 periods per second it is only 104° .

Table II shows that maximum phase-displacement occurs at lower surface induction density the lower the frequency. This is also the case in Table III with alternating magnetic force.

It is unnecessary to publish all the curves of electromotive force obtained. Figs. 8, 9, and 10 refer to a periodic time of 45 seconds, and the corresponding values of induction density can be seen in Table II. Fig. 8 gives the initial stages with small external magnetic force in which the phase-displacements are relatively small, and the No. 1 and 2 electromotive forces are important. In fig. 9, the electromotive forces of Nos. 1 and 2 coils are relatively smaller, and the phase-displacements are large. In fig. 10, with high external magnetising force, the electromotive forces are in phase and of equal relative importance.

It is interesting to note the relation between the average permeability of the iron cylinder, and the rate at which the magnetism is propagated to the centre. When the permeability is small the effects penetrate rapidly. With large permeability in the iron the centre is reached with greater difficulty, hence the diminished value of the induction density at the centre and the increased phase-displacement. With the high limits of induction density the average permeability is small and the effects can penetrate more rapidly.

With intermediate magnetic force at 22.5 seconds periodic time, the electromotive force of No. 1 coil is diametrically opposed to that of No. 3 coil. That is to say, the intensity of induction at the centre of the cylinder is in the same direction as near the surface but of reversed sign.

IV. *Longitudinal Variation of Intensity of Magnetic Induction.*—The preceding remarks refer to the variation of intensity of induction over a section of the cylinder at its centre 2 inches (5.08 cms.) thick. We have seen how the magnetism is propagated over this section radially towards the centre. It is necessary to deal with the variation of induction density as one proceeds from either end of the cylinder to its centre.

The electromotive forces of each of the coils I, II, III (fig. 1) were observed simultaneously as in the case of coils 1, 2, 3, and by integration the maximum average intensity of magnetic induction over each area has been found. An examination of these curves shows that the effects of induced currents penetrate from each end of the cylinder

along area I in much the same way as was observed radially—that is to say, the intensity of induction diminishes in value and suffers retardation in phase. Table IV gives the values of the induction density and the phase-displacements for periodic times of 360, 90, and 45 seconds. In order to compare the diminution of induction density as one proceeds along the axis of the cylinder towards the centre, the maximum average values of induction density over Sections 1 and I have been plotted in figs. 2, 4, 6. The points are marked $\times$ and agree fairly well with the corresponding curves. The phase-displacements are shown in Table IV, and are a maximum for the intermediate force. Comparing the maximum average values of the intensity of induction over Sections 2 and II, we see that the corresponding differences are not so great and the phase-displacements are less. The maximum average values of induction density over Sections 3 and III are more nearly equal and the phase-displacements are small. Lastly, comparing the maximum average intensities of induction over areas I, II, III, we see that the diminution is not so great as over the areas 1, 2, 3, and the corresponding phase-displacements are less.

V. We have seen that with a cylinder of 10 inches axial length, the induced currents do not seriously disturb the approximately uniform distribution of induction density over its section, when rotated in a magnetic field with periodic time 360 seconds. This periodic time corresponds to a frequency of 179 periods per second in a cylinder of axial length 0.1 cm. In ordinary dynamo-electric machines the frequencies usually met with are very much smaller than 179, and plates 0.1 cm. thick are used. The inference is, that in cases in which as good an approximation to a pure rotating field as has been obtained in these experiments is met with, no serious deviation from uniform distribution exists, provided the plates are insulated from one another.

VI. Induction motors for a certain class of work are now supplied with solid iron armatures. Suppose such a motor with an armature 10 inches diameter and 10 inches long, and with one period per revolution, is placed on a supply system having a frequency of 30 periods per second. Fig. 6 refers to a periodic time of 45 seconds, or a frequency of 0.022 period per second. To obtain the effects observed at this frequency the armature would have to rotate at frequency 29.978—that is, the effective frequency or “slip,” as between the rotating field, which would have to be as uniform as in these experiments, and the armature is 0.073 per cent. of the frequency of supply. To obtain the effects observed at 22.5 second periodic time, the effective frequency would be 0.146 per cent. of the frequency of supply. A slip of 5 per cent. could easily be obtained in practice, and this would correspond to an effective frequency sixty-eight times

as great as in fig. 6. Apart from phase-displacements we see that the interior portions of the cylinder would be quite useless with regard to induced magnetism, unless very large or very small external magnetising forces were employed.

VII. Experiments were made upon a steel wire 0.01 inch (0.00254 cms.) diameter, the object being to discover if a true time lag in magnetism exists.* Frequencies of 5, 72, and 125 periods per second were tried, and the results obtained were compared with the curve of magnetic hysteresis obtained by the Ballistic-galvanometer method. The curves so obtained would indicate a somewhat higher dissipation of energy with the high frequencies. Now the effects shown in fig. 3 would also be observed in a wire 0.01 inch diameter if the frequency were 600. The inference is that at 125 periods per second there would still be disturbances due to induced currents. The author inclines to the opinion that the effects associated with time lag in magnetism may be due to induced currents in the wire.

VIII. Lord Kelvin has computed that the earth's magnetism is travelling in the direction of the sun round the earth with a periodic time relatively to the earth of 960 years. The magnetic declination at London had an amplitude of $24^{\circ} 34' 30''$ W. in 1820. Diurnal variation is observed to the extent of some minutes of arc. The reaction of the induced currents in the cylinder produces a displacement of polarity. In a cylinder similar in all respects to the one experimented upon, but having a diameter equal to that of the earth, a periodic time of 960 years would produce similar magnetic and electric events as would be observed in the 10-inch cylinder if the latter could be rotated with a periodic time 12×10^{-6} second. This is nearly 2,000,000 times as fast as the fastest speed in these experiments. Diurnal changes in spite of small magnetic force would produce effects confined to the surface of such a cylinder, and would not appreciably disturb an effect due to a periodic time of 960 years. Table II shows that when the intensity of magnetic induction at the surface of the 10-inch cylinder has the value 169, phase differences of the order 44° ($360^{\circ} = 1$ period) are experienced with a periodic time of 45 seconds. The magnetic force H per centimetre linear in C.G.S. units would be about 0.5 in this iron, giving an average permeability of about 280. This is a force of the same order as the earth's total intensity.

IX. If we were to work upon smaller and smaller masses of iron until we reached the limit at which all the properties of the original mass were still preserved, we should require enormous speeds of rotation to produce the disturbances examined in this paper. Suppose the diameter of such a mass were of the order 0.000,0001 cm., then to produce similar effects to those which we have observed at 90 seconds

* See Hopkinson, Wilson, and Lydall, 'Roy. Soc. Proc.', vol. 53, p. 332.

periodic time, such a body would have to rotate at 7×10^{14} revolutions per second.

I wish to thank Messrs. Siemens Bros. and Co. for the loan of the field magnet, which was altered for the purpose of these experiments.

I have pleasure in acknowledging the help I have received from Mr. F. S. Robertson and Messrs. H. A. Skelton, R. M. Wartze, and M. S. Duffitt. Mr. Skelton has spent a great deal of time in the working out of the results. Mr. T. Jones has also rendered valuable assistance.

Table I.

Diameter and length each 25.4 cm.	Diameter and length each 12.7×10^3 cm.	Diameter and length each 0.1 cm.
Periodic time.	Periodic time.	Frequency.
360 seconds	—	179 periods per second.
90 "	7×10^9 years	} 717 " "
45 "	2.2×10^{17} seconds	
12×10^{-6} seconds	—	1434 " "
1.3×10^{-8} "	960 years.	
8.5×10^{-11} "	8.03×10^{10} seconds.	
	1 year.	
	3.1×10^7 seconds.	
	1 day.	
	8.6×10^4 seconds.	

Table II.—Rotating Magnetic Force.

Periodic time in seconds.	Frequency for a cylinder of diameter and length 0.1 cm.	Maximum average induction B per sq. cm.				Phase-displacement of E.M.F. relative to E.M.F. of No. 3 coil. 360° = 1 period.			Figure.
		Centre. Coil 1.	Radius 2 inches. Coil 2.	Radius 4 inches. Coil 3.	Over whole section. Coil 4.	Coil 1.	Coil 2.	Coil 4.	
22.5	2806	234	282	1,807	1,177	103	57.6	3.2	} 8 9 10
"	"	373	1,109	10,770	6,350	176	96	12.8	
"	"	1,504	6,530	14,840	10,600	186	115	23.4	
45	1431	124	128	169	137	44	19	11	
"	"	371	486	1,356	959	69	36	5.6	
"	"	426	629	2,710	2,080	80	40	5.8	
"	"	714	1,037	5,645	3,730	86	43	5	
"	"	738	1,760	10,900	6,840	119	66	3.8	
"	"	7,830	8,780	13,800	11,800	123	58	3.8	
"	"	14,260	15,460	17,000	16,570	15	13	4.8	
"	"	17,500	19,510	18,800	20,400	2	2	5.5	
"	"	19,210	21,230	21,660	23,020	-1.6	-1.6	-2.2	
90	717	116	155	163	147	34	18.4	3.4	} 4
"	"	894	1,115	2,085	1,705	43	21	1	
"	"	916	1,204	2,380	1,883	47	25	3.7	
"	"	1,174	1,901	4,890	3,540	53	23	3.2	
"	"	2,090	4,540	8,586	7,360	88	40	14	
"	"	4,164	6,342	9,757	8,456	104	46.5	14	
"	"	12,570	13,810	14,800	14,600	14	10	1.6	
"	"	15,750	17,250	18,000	17,200	2.1	4.6	0	
"	"	18,000	19,400	18,780	20,000	3.5	4	8	
"	"	19,400	21,050	21,480	21,700	0	2.4	0	

Table II—continued.

Periodic time in seconds.	Frequency for a cylinder of diameter and length 0.1 cm.	Maximum average induction B per sq. cm.				Phase-displacement of E.M.F. relative to E.M.F. of No. 3 coil. $360^\circ = 1$ period.			Figure.
		Centre. Coil 1.	Radius 2 inches. Coil 2.	Radius 4 inches. Coil 3.	Over whole section. Coil 4.	Coil 1.	Coil 2.	Coil 4.	
180	358	1,168	1,389	1,525	1,850	21	9.6	1.6	2
"	"	2,348	3,018	4,220	3,680	25.5	12.8	6.4	
"	"	6,160	6,830	8,710	7,790	51.8	22.8	2.6	
"	"	10,000	11,000	12,040	11,200	27.8	13.2	5.3	
"	"	15,050	17,000	16,690	16,480	8.2	4	0	
"	"	18,180	20,090	20,500	20,470	0.5	1.3	0	
"	"	19,500	21,130	21,600	21,570	0	0	0	
360	179	1,199	1,193	947	951	21	12	8	
"	"	2,303	2,274	1,810	1,970	13.6	12.8	2.3	
"	"	5,290	5,711	5,159	5,233	17.5	10.4	4	
"	"	7,670	8,180	7,170	6,970	20	11.2	4.5	
"	"	11,470	11,600	11,290	11,630	8.8	6.0	3.2	
"	"	15,400	17,200	18,600	18,400	2	1.2	2.8	
"	"	18,000	19,800	19,110	20,000	4.8	8.6	1.7	
"	"	18,600	20,740	21,010	21,470	0.8	1.6	1.6	

Table III.—Alternating Magnetic Force.

Periodic time in seconds for a cylinder 12 inches diameter.	Fre- quency for a cylinder 0.1 cm. diameter.	Max. H due to magne- tising cur- rent in cop- per coils on magnet.	Maximum average induction B per sq. cm.					Phase-displacement of E.M.F. relative to E.M.F. at radius 5 inches. 360° = 1 period.			Figure.
			Centre.	Radius 1.5 inches.	Radius 3 inches.	Radius 5 inches.	Over whole section.	Centre.	Radius 1.5 inches.	Radius 3 inches.	
618	150	1.10	965	--	--	1,340	1,200	79	--	--	3
"	"	2.82	2,950	3,630	5,140	5,670	5,085	103	86	45	
"	"	9.60	12,600	11,900	14,800	14,000	14,140	69	55	31	
"	"	16.70	15,300	14,400	16,900	16,900	17,870	41	32	19	
312	300	1.10	661	--	--	1,565	1,260	48	--	--	5
"	"	9.45	11,500	11,150	13,700	13,860	13,020	115	88	49	
156	600	1.10	362	--	--	1,280	884	67	--	--	
"	"	2.88	523	680	1,980	5,540	3,617	118	91	55	
"	"	9.51	2,310	5,200	10,670	13,300	10,420	168	122	61	5
"	"	16.9	14,400	13,800	15,400	15,500	15,500	104	80	41	

Table IV.

Periodic time in seconds.	Maximum average induction B per sq. cm.						Phase-displacements. $360^\circ = 1$ period.					
	Centre. Coil I.	Radius 2 inches. Coil 2.	Radius 2 inches. Coil II.	Radius 4 inches. Coil 3.	Radius 4 inches. Coil III.	Coils 3 and 1.	Coils 3 and 2.	Coils I and 1	Coils II and 2.	Coils III and 3.	Coils III and I.	Coils III and II.
45	470	592	840	1,692	1,980	70	35	58	34	7	16	8
"	864	2,320	5,430	10,300	11,040	118	64	93	46	7	32	26
"	18,800	19,500	18,900	18,800	20,000	2	2	3	2	4	3	4
90	933	1,210	1,550	2,140	2,340	47	22	36	15	0	12	7
"	3,386	5,300	6,990	9,760	10,000	101	45	70	33	-3	26	9
"	18,800	19,300	19,500	18,650	19,500	1.5	3	0	9	1.5	3	-4
360	8,260	7,980	6,780	6,580	6,350	18	12	16	11	2.4	4.4	3
"	18,500	20,000	19,650	19,110	19,320	0	0	0	0	0	0	0

"Note on Mr. Bateson's Paper, 'Heredity, Differentiation, and other Conceptions of Biology: a Consideration of Professor Karl Pearson's Paper "On the Principle of Homotyposis."'"
By KARL PEARSON, F.R.S. Received and Read February 20, 1902.

A paper under the above title by Mr. W. Bateson was read on February 14, 1901, and appears on pp. 193—205 of the Society's 'Proceedings,' vol. 69. It naturally calls for a reply from me, for Mr. Bateson's criticism attacks the fundamental ideas at the basis of the long series of papers on "The Mathematical Theory of Evolution" which have been contributed by me to the 'Transactions' and 'Proceedings' during the last eight years. My reply must therefore not only be lengthy, but discuss the validity of the opposed terms and definitions used by the school of evolutionists of which Mr. Bateson is one of the most noteworthy representatives. Naturally, Mr. Bateson will want a rejoinder, and this might require a counter-rejoinder. Somewhere, however, the controversy must be ended, or its *renu-* changed. Without questioning the right of Mr. Bateson to choose the 'Proceedings' of the Society for the publication of his adverse criticisms, I must state my own individual opinion that a full discussion of this matter must be long and argumentative, because it goes to the foundation of more than one hypothesis warmly advocated by this or that school of biologists.

I believe that discussions of this kind, highly controversial in character, however much they tend to clear up scientific ideas and enforce rigidity in use of terms, are not contributions to original knowledge, and so are far better carried on elsewhere than in the publications of the Society. For this reason, I propose to transfer my reply to Mr. Bateson's criticism to another place. I hope this brief note may explain to Fellows of the Society that it is through no discourtesy that I animadvert elsewhere on the interesting points which Mr. Bateson has recently brought to their notice.

February 20, 1902.

Sir WILLIAM HUGGINS, K.C.B., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

- I. "On Pure Cultures of a Uredine, *Puccinia dispersa* (Eriks.)." By Professor MARSHALL WARD, F.R.S.
- II. "On the Physics and Physiology of Protoplasmic Streaming in Plants." By Dr. A. J. EWART. Communicated by Professor GOTCH, F.R.S.
- III. "On a Pair of Ciliated Grooves in the Brain of the Ammocoete, apparently serving to promote the Circulation of the Fluid in the Brain-cavity." By Professor A. DENDY. Communicated by Professor HOWES, F.R.S.
- IV. "On the Interpretation of Photographic Records of the Response of Nerve obtained with the Capillary Electrometer." By G. J. BURCH, F.R.S.
- V. "Note on the Anomalous Dispersion of Sodium Vapour." By Professor W. H. JULIUS. Communicated by Professor C. V. BOYS, F.R.S.
- VI. "Note on Mr. Bateson's Paper, 'Heredity, Differentiation, and other Conceptions of Biology: a Consideration of Professor Karl Pearson's Paper "On the Principle of Homotyposis."'" By Professor KARL PEARSON, F.R.S.

"On Pure Cultures of a Uredine, *Puccinia dispersa* (Eriks.)."
By H. MARSHALL WARD, F.R.S. Received February 5,—
Read February 20, 1902.

During the course of a long series of experiments, designed to seek further information regarding the relations between the host and the parasite, in the case of certain species of *Bromus* and the Rust-fungus, *Puccinia dispersa* (Eriks.), it fell out that a simple method of obtaining pure cultures of both host and parasite was attainable.

Tube-cultures.

In order to obtain more decisive answers to such questions as—Are any of the results obtained on plants in the open, or merely covered with bell-jars and so forth, due to spores accidentally introduced, or to mycelium, &c., already in the plant?—a number of infections were made on seedlings germinated and grown anti-septically in tubes as follows:—

As a preliminary trial, to test whether grass seedlings would live long enough in tubes and show the results of infection, I placed seedlings, all similar, and bearing three to four leaves, in test-tubes, the roots, carefully washed free of soil, resting on wet cotton wool, and the orifice of the tube plugged. The results of two series apparently showed that spores from *B. mollis* infect *B. mollis* and *B. sterilis*,* *B. secalinus* and *B. arvensis*, but not *B. inermis*, while spores grown on *B. sterilis* only infected *B. secalinus* and *B. arvensis* (see Table I).

It is a striking fact that under the conditions afforded by these closed tubes, with their stagnant moist atmosphere, spores from *B. sterilis* apparently succeeded in infecting *B. secalinus* and *B. arvensis*. The pustules were small, and it is significant that it took 14 days' incubation to establish the mycelium, against 9 days in the case of spores from *B. mollis*.

There are, however, other slight departures from the usual course of events—e.g., spores from *B. mollis* failed on *B. arvensis* and succeeded on *B. sterilis*—which may point to the probability that these uprooted seedlings in tubes are not in their normal conditions. The facts seem worth recording in this connection.

I varied these tube-cultures by placing them, after infection, under cuvettes filled with copper sulphate (blue light) or bichromate of potassium (orange light). The results were somewhat contradictory as regards the blue, but it did appear that the spores coming from *B. mollis* can infect *B. sterilis* as well as *B. secalinus*, as the following Table II shows.

It is important here to bear in mind the origin of the seedlings themselves, however, as a serious source of error was discovered in these uprooted seedlings, previously exposed to possible infection in the open.

The same criticisms, in fact, apply to these transplanted garden seedlings in tubes as I have given elsewhere regarding such experiments in pots, viz., it was always possible that the seedling was already infected, or had wind-blown spores on it when removed from the beds. At the same time the long incubation period, 14 days in some cases, fails to support this.

* An exceptional circumstance, as numerous experiments have shown, and itself sufficient to raise the criticism which follows.

Table I.—Three-leaved Seedlings in closed Test-tubes, on wet Cotton-wool, kept at N. Window and infected forthwith with Spores from *B. mollis* and *B. sterilis*.

Exp. No.	Date.	Host.	Origin of spores.	Result.*	Incubation period.	Period of experiment.	Remarks.
697	May 25	<i>B. inermis</i>	<i>B. sterilis</i>	—	..	21 days	Leaf yellow and faded by June 3.
"	"	<i>B. sterilis</i>	"	—	..	"	
"	"	<i>B. mollis</i>	"	—	..	"	
"	"	<i>B. secalinus</i> ...	"	+	14 days	"	Pustules very small.
"	"	<i>B. arvensis</i>	"	+	"	"	" "
705	May 30	<i>B. mollis</i>	<i>B. mollis</i>	+	9 days	"	Two leaves still green at end of experiment.
"	"	<i>B. arvensis</i>	"	—	..	"	
"	"	"	"	+	9 days	"	
"	"	<i>B. inermis</i>	"	—	..	"	Two green leaves left at end.
"	"	<i>B. sterilis</i>	"	+	9 days	"	
"	"	<i>B. secalinus</i>	"	+	"	"	

* In all cases the sign + signifies that spore-bearing pustules were developed; the sign — that no signs of infection were visible.

Table II.—Test-tubes with Seedlings (as in Experiment 697) infected on Third Leaf and then put under Cuvettes of CuSO_4 and K bichromate respectively, and kept in the garden fully exposed to sun during mid-day.

Exp. No.	Date.	Host species.	Origin of spores.	Result.	Incubation.	Colour of light.	Remarks.
698	May 25	<i>B. sterilis</i> ..	<i>B. mollis</i>	+	14 days	Orange-red	
"	"	" ..	"	+	"	"	
"	"	" ..	"	—		Blue	No trace of pustules, though two leaves still green.
"	"	" ..	"	—		"	" " "
709	June 11	" ..	"	+	10 days	Orange-red	
"	"	" ..	"	+	11 "	"	
"	"	" ..	"	+	11 "	"	
"	"	" ..	"	—	11 "	Blue	
"	"	" ..	"	—	11 "	"	
"	"	<i>B. secalinus</i>	"	+	11 "	"	
"	"	" ..	"	+	11 "	"	
"	"	" ..	"	—	"	"	
"	"	" ..	"	+	10 "	Orange	
"	"	" ..	"	+	10 "	"	

On the 23rd, however, I note that the cuvette leaked above and has let in white light.

To meet this objection I devised operations as follows:—

Clean picked seeds were placed singly, by means of forceps, on filter paper at the bottom of Petri-dishes properly sterilised by heat. When these had germinated, and observation showed that the whole series was free of moulds or other signs of contamination, the seedlings were removed by means of sterile forceps, and transplanted singly into sterilised tubes of various kinds as described below, and the further growth allowed to proceed in the light under conditions varied as will be seen.

I had already shown that seedlings will continue to grow in such tubes, but, as we have seen, in the cases previously described I had no guarantee that the seedlings introduced into the culture-tubes did not already carry on their leaves wind-borne or otherwise transmitted spores.

In the case of these seedlings germinated from clean "seed" in sterile dishes and tubes, it is obvious that the only chance of infection depends on spores attached to the "seed" or on mycelium in the seed.

Experiments with seed gathered even from badly rusted plants and germinated as above, have never given rusted seedlings, although other experiments have shown me that the germ-tubes of attached spores can infect seedlings when the plumule is only 3—5 mm. high. Nor have I ever been able to discover any trace of mycelium in the seeds.

But if the "seed" of the *Bromus* is sterilised before germination—as can be done by steeping in various antiseptics, or by heating to 60—70° C.—it is found that pure cultures of the Brome may be obtained in the tubes, and it is then only necessary to infect such a clean seedling with the spores of the parasite to obtain a pure culture of the latter.

Preliminary experiments soon showed that the Brome seedlings thus raised from seeds treated antiseptically, and protected from the first by glass, may be grown for weeks and even for a couple of months in such tubes under proper precautions, and I set myself the task of ascertaining how such cultures would behave in infection experiments.

In the following experiment—No. 711—eight upright tubes of the kind known to chemists as "drying towers," were prepared as in the diagram (fig. 1), so that by means of an aspirator attached to the tubing at G, a continuous current of damp air could be slowly drawn through the whole series, aerating the roots of the seedlings F, which burrowed into the cotton-wool B, day and night. The tubes were charged each with one seedling, grown from seeds heated to 65° C., and 48 hours after germination had begun, and the latter allowed to grow in the light on a table outside the laboratory. The tubes were charged on June 14, and on June 19, when the first green leaf was well developed, the latter was infected at a definite spot with spores—proved

to be capable of vigorously germinating by cultures in hanging drops—and the whole series linked up and aerated. The growth of these seedlings in the moist air-current was very satisfactory, the plants

FIG. 1.

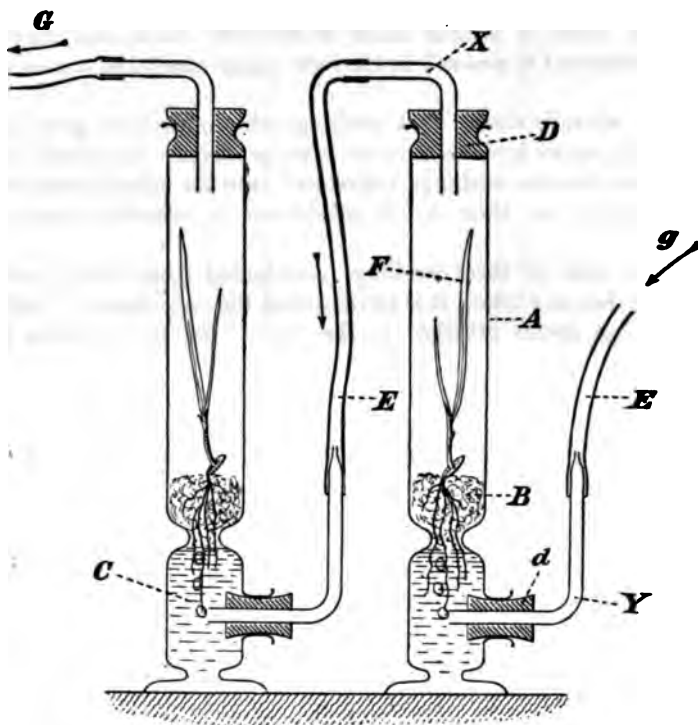


Diagram showing arrangement of tubes for pure cultures of grass seedlings—see text—here shown connected up for aëration. Reduced.

A = glass jar. B = cotton-wool saturated with liquid. C = liquid reservoir containing nutritive culture solution, through which air bubbles pass. D and d = caoutchouc stoppers pierced by glass tubing. EE = caoutchouc tubing. F = seedling with its roots in B and its leaves in air. G and g = arrows showing direction of air current. x and y = glass tubes.

having a deep rich green colour, though the leaves were short, and the results, as shown in the following Table III, were very instructive.

In this series the liquid employed was the normal Knop's mineral solution (+), so well known as used in water-cultures. The tubes were charged with this before sterilisation, enough being put in to wet the cotton-wool plug (B), and fill the reservoir (C), the side-tube y being used at its pointed end during sterilising.

Table III.—Experiments in Aërated and in Closed Tubes. Selected and Sterilised Seeds and Clean Seedlings.
Infected when one week old. Roots in Knop's Solution.

Expt. No.	Date.	Host.	Origin of spores.	Treatment.	Results.	Period of incubation.	Period of experiment.	Remarks.
711	June 19	<i>B. sterilis</i> . . .	<i>B. mollis</i>	Aërated continuously	—	12 days	21 days	
"	"	<i>B. mollis</i> . . .	"	"	+	16 "	"	
712	"	<i>B. sterilis</i> . . .	"	"	+	"	"	
"	"	" . . .	"	"	—	"	"	
"	"	<i>B. mollis</i> . . .	"	"	?	"	"	Flecks developed, but no spores formed.
"	"	" . . .	"	"	?	"	"	"
713	"	" . . .	"	Closed tubes	+	8 "	"	
"	"	<i>B. sterilis</i> . . .	"	"	+	12 "	"	
"	"	" . . .	"	"	—	"	"	
"	"	<i>B. relativus</i> . . .	"	"	—	"	"	
"	"	<i>B. marinus</i> . . .	"	"	+	10 "	"	Very fine growth of sporiferous pustules.
"	"	<i>B. madritensis</i> . . .	"	"	—	"	"	
"	"	<i>B. commutatus</i> . . .	"	"	—	"	"	
"	"	<i>B. areolaris</i> . . .	"	"	+	10 "	"	
"	"	<i>B. secalinus</i> . . .	"	"	+	8 "	"	Very fine pustules.
"	"	<i>B. interruptus</i> . . .	"	"	+	10 "	"	
"	"	<i>B. racemosus</i> . . .	"	"	+	11 "	"	
"	"	" . . .	"	"	+	"	"	

Since each tube is linked to its neighbour with clean flexible tubing, and the air bubbles through the liquid in the reservoir (C), and has to pass the cotton-wool plug (B) before reaching the leaves (F) in the air above, there can be no question of infection from outside, and the results also show that *infection only occurs exactly where I place the spores on the leaf in each case.*

The spores employed were carefully tested as regards their germinating power, and, as the table shows, the results in the closed tubes fully bear out previous experience. In the aspirated tubes, however, the second pair of seedlings of *B. mollis* (No. 712) gave negative results, inasmuch as only flecks, and not pustules bearing spores, were developed. In the closed tubes, however—see below—the positive results, especially on *B. velutinus* and *B. secalinus*, were excellent, and subsequent examination showed that the spores germinated well, and were capable of infecting other seedlings.

In order to test further the behaviour in mineral solutions, I prepared, as Table III shows, several series in closed tubes, Nos. 713, which served as a parallel series to Nos. 711 and 712, but without aëration.

In No. 713 the sterile seedlings were raised antiseptically as before, but the roots merely penetrated cotton-wool saturated with Knop's solution, and held by the constriction over the bulb filled with the same, no air being drawn through.

The growth was excellent, and the results very conclusive, as Table III shows.

The seedlings were allowed two days at 22—20° C. in the laboratory and then put out side by side with 711 and 712 in full sun during the middle of the day, and after two days' further growth were infected.

By the 10th day the thin leaf was well developed, and the first pustule was seen on *B. mollis* and *B. secalinus* on the eighth day after infection. The Table III shows the course of events in detail.

The growth of pustules was excellent on *B. velutinus* and *B. secalinus* especially.

This experiment is interesting, not only as showing that plants can be grown and infected successfully in these closed water-cultures, but especially as showing the contrast between the aërated and non-aërated tubes, for, since the infected seedlings were selected in each case from the same Petri-dish cultures, we must assume that the difference in rate of development was due to the difference of ventilation, and perhaps conclude that this interferes with the success of the parasite, as measured by the somewhat longer incubation period. It is remarkable how dwarfed the continuously aërated plants are, compared with those in closed tubes, owing to the elongation of the leaves of the latter.

It is clear, therefore, that pure cultures of Uredo-spores can be obtained by this method, and it is equally clear that we can also obtain

pure cultures of the host-plants, and since we can do this, there is no reason why the infection of Uredineæ should not be conducted as rigorously and exactly as that of bacteria. As matter of fact, I have succeeded in proving that it can,* though of course the length of time occupied in a large series of cultures and infections will prove troublesome, and it remains to be seen whether we can get such plants to flower.

The following Table IV gives the results of an additional series where *B. secalinus* was the host, and spores from the same used as as infecting material. In this case the tubes used were closed from the first (see fig. 2). These results appear to me to dispose effectually of the question as to whether or not pure cultures of Uredineæ can be obtained.

A number of isolated tube-cultures were made with spores from *B. sterilis*, *B. mollis* and *B. secalinus*, and arranged similarly, and confirmatory results obtained. Moreover, I was able in several cases to transfer successfully spores from these pure tube-cultures to other tubes of pure cultures of seedlings, and to prove that the spores raised under strictly antiseptic conditions are capable of germination and infection.

At the same time, it was noteworthy that in several cases the antiseptically raised spores were not always successful in infecting the seedlings, and it remains for further investigation to determine whether this was due to the conditions of culture of the fungus or the host or both.

Experiments with Mineral Supplies.

The question arose during some of the tube-experiments, Do infection and incubation occur normally if the host-plant is suffering from a lack of any particular ingredient in the soil—e.g., one of the essential minerals? To answer this, I prepared tube-cultures as before—as in experiments Nos. 711 and 713, &c.—substituting for the normal nutritive solution, in some of the tubes, the same solution minus calcium, or minus potassium, or magnesium or phosphoric acid, respectively, with the results shown in the following Table V. The only positive result was with spores from *B. mollis*, which infected *B. mollis* in spite of the lack of Mg salts. In other cases flecks only were developed.

This experiment cannot be regarded as very decisive, but it at least shows that infection *can* occur when Mg is lacking, though the incubation period (13 days) was somewhat long. As the pot-cultures show, no positive results were to be expected with *B. maximus* infected from *B. mollis*.

* I.e., of course so far as fungi are concerned: the antiseptic treatment adopted does not always exclude harmless bacteria.

TABLE IV.—Closed Tubes of Antiseptic Seedlings (Seeds Sterilised at 65° C.) in Nutritive Solutions. Tubes merely Plugged, and Seedlings on wet Plug with Solution below. Kept in moderate Sunshine.

No.	Date.	Host.	Origin of spores.	Nutritive solution.	Aerated or not.	Results.			Remarks.
						July 29.	July 30.	July 31.	
772 a.....	July 19.	<i>B. cereolina</i> .	<i>B. cereolina</i> .	Pfeffer normal	not	+	+	+	
" b.....	"	"	"	"	"	0	+	+	
" c.....	"	"	"	"	"	+	+	+	
" d'.....	"	"	"	"	"	0	0	+	
" d.....	"	"	"	"	"	+	+	+	
" d'.....	"	"	"	"	"	+	+	+	
" e.....	"	"	"	Pfeffer without Fe	"	+	+	+	
" e'.....	"	"	"	"	"	+	+	+	
" f.....	"	"	"	Detmer, without K	"	+	+	+	
" f'.....	"	"	"	"	"	0	0	0	
" g.....	"	"	"	Detmer, without P	"	+	+	+	
" g'.....	"	"	"	"	"	0	+	+	

The following experiment (Table VI) seems at first sight very suggestive in its definite results, but much more work will be needed before we can generalise widely. Five tubes with bulbs below were charged with a seedling each of *B. arvensis* and five with *B. velutinus*, one of each five tubes having normal Detmer's solution, one the same minus K, Ca, &c., respectively. The seedlings were 10 days old, grown antiseptically on moist filter paper, and therefore exhausting the reserves, and would probably come at once under the full influence of the mineral solutions.

Each seedling was infected forthwith with spores from *B. mollis*, tested, and found to be not very vigorous in germination. Since all were infected alike, however, on the first leaf and from the same batch of spores, I think we must regard the results as fairly comparable.

As the table shows, every plant of *B. velutinus* had spore-bearing pustules in 10—11 days, whereas the *B. arvensis* gave no signs, if we except one doubtful pustule on the 11th day in the tube devoid of potassium.

It seems necessary to conclude from this series that *B. arvensis* is far less easily infected than *B. velutinus* under any conditions of mineral supply afforded. The experiment also shows that in the case of a susceptible plant such as *B. velutinus*, no appreciable effect is produced by the mineral supply in the short time (10—11 days) occupied in incubation. Something may turn out to be due to the differences in size of the seeds, however.

Meanwhile I had made some experiments on seedlings started in pots of coir-fibre, under conditions more



FIG. 2.—Pure culture of *Puccinia dispersa* on *Bromus velutinus*. The "seed" of the grass, antiseptically sterilised as regards fungus spores by heating to 65° C., was germinated in the sterile tube, and infected on the first leaf with spores developed on *Bromus mollis*. The infection was successful, and pustules of spores have appeared *only* on the area inoculated. About natural size.

Table V.—Large Tube-cultures of Seedlings, (Germinated on Damp Filter-paper for six days, and then placed with their Roots in Mineral Solutions, and infected two days later.

Expt. No.	Species.	Origin of spores.	Nutritive solution.	Result.	Period of incubation.	Duration of experiment.	Remarks.
714	<i>B. mollis</i>	<i>B. mollis</i>	Water only.....	—	13 days	21 days	A yellow fleck. * Plant lost. A fleck. Flecks.
"	"	"	Normal Detmer*..	—		"	
"	"	"	Minus Ca.....	—		"	
"	"	"	" K.....	—		"	
"	"	"	" Mg.....	—		"	
"	"	"	" P ₂ O ₅	—		"	
"	<i>B. maximus</i> ..	"	Water only.....	—		"	
"	"	"	Normal Detmer...	—		"	
"	"	"	Minus Ca.....	—		"	
"	"	"	" K.....	—		"	
"	"	"	" Mg.....	—		"	No infection anywhere visible.
"	"	"	" P ₂ O ₅	—		"	

Table VI.—Exactly as 715, but Seedlings ten days old and already exhausting their reserves.

Expt. No.	Species.	Origin of spores.	Nutritive solution.	Result.	Period of incubation.	Duration of experiment.	Remarks.
721 & 722	<i>B. arvensis</i> ...	<i>B. mollis</i> ...	Normal Detmer...	—	11 days?	21 days	{ Fleck only, or at most bearing a few spores.
"	"	"	Minus Ca.....	—		"	
"	"	"	" K.....	—		"	
"	"	"	" Mg.....	—		"	
"	"	"	" P ₂ O ₅	—		"	
"	<i>B. velutinus</i> ..	"	Normal Detmer...	—		"	
"	"	"	Minus Ca.....	—		"	
"	"	"	" K.....	—		"	
"	"	"	" Mg.....	—		"	
"	"	"	" P ₂ O ₅	—		"	

nearly approaching the normal than, perhaps, the tubes afford, to see if the influence of mineral supplies, or lack of such, at the roots affects the questions before us.

Seeds of four species of *Brome* were sown in pots of coco-fibre (coir) thoroughly steamed in a steriliser. The seedlings—all except *B. asper*—came up satisfactorily, but did not form strong plants eventually. As the following Table VII shows, infection failed in all cases, in spite of the fact that the seedlings were given a double dose of spores. It is difficult to explain the negative results with *B. mollis*. The temptation is to conclude that the host-plant, being starved of mineral supplies, gave the infecting germ-tube no chance to establish itself, but I think much more work must be done before this is established.

The following experiments, however, yielded results which seem to show that the nature or amount of the mineral supply may after all be not without effect.

Small pots were packed with washed coco-nut fibre and sterilised by steam, and "seeds" of *B. mollis* sown in twelve of them. Germination was started at 27° C. in the artificial greenhouse and the pots then placed in a well-lighted, ordinary greenhouse and watered as follows:—Two with Detmer's normal solution, and two each with the same minus K, Ca, Mg, and P_2O_5 respectively, while two were kept as controls watered with distilled water.

On the 5th day the spears were up and the first leaf unfolding, and one pot of each pair mentioned infected with spores from *B. mollis*. The parallel set were similarly infected, with spores from *B. mollis*, two days later, a different crop of spores being used.

After four days' incubation under wet beakers—in the case of the first six pots—and two days—in the case of the second six—all were put out side by side in a sunny place under a large bell-jar. Two days later the bell-jar was removed. The pots were placed well apart, each in its own saucer, and equal carefully measured doses of the minerals given at intervals.

The seedlings grew admirably at first, but began to show the effects of the mineral deficiency in a few days. Meanwhile the incubation period was being completed. The results appear in Table VIII. There were, however, very marked points of difference between the appearances in this series 716 and those seen in perfectly normal plants—*e.g.*, series 735–736 and 738.

In the first place, none of the pustules or disease-flecks were very large or well developed. They were, curiously enough, best on the seedlings provided with water only. Next best on those with normal mineral solution, and on those with the same devoid of Ca, and (another curious fact) in one case on the plants deprived of Mg. On those deprived of P_2O_5 or of K the development was distinctly worse.

Table VII.—Seedlings sown in Coco-fibre on May 24. Infected May 29, and re-infected May 30, in each case in Tip-drops. Water only used to keep the Coir damp.

Experi- ment No.	Date of infection.	Host.	Origin of spores.	No. of plants.	No. inocu- lated.	Results.			Remarks.
						July 8.	July 11.	July 15.	
702 (1)	May 29	<i>B. inermis</i> ..	<i>B. mollis</i> ...	25	10	0	0	0	
" (2)	"	"	" ...	"	"	0	0	0	
" (3)	"	<i>B. mollis</i> ...	" ...	"	"	0	0	0	
" (4)	"	" ...	" ...	"	"	0	0	0	
" (5)	"	<i>B. sterilis</i> ..	" ...	"	"	0	0	0	
" (6)	"	"	" ...	"	"	0	0	0	
" (7)	"	<i>B. asper</i> ...	" ...	"	"	..	..	..	} Did not come up.
" (8)	"	"	" ...	"	"	..	..	..	

Table VIII.—Seedlings in Sterilised Pots of Coco-fibre, saturated with Mineral Culture Solutions, and infected after 5—7 days, in Greenhouse.

Expt. No.	Date of sowing.	Host.	Origin of spores.	Nutritive solution.	Date of infection.	Result.	Period of incubation.	Duration of experiment.	Remarks.
716 a	June 17	<i>B. mollis</i>	<i>B. mollis</i>	Water only.....	June 22	+	10 days	24 days	Excellent plants and pustules.
" b	"	"	"	Normal Detmer	"	+	11 "	"	Rather pale, but good pustules.
" c	"	"	"	Minus Ca.....	"	+	12 "	"	Fair plants; pustules small.
" d	"	"	"	" K	"	+	12 "	"	Some leaves shrivelled; pustules few and small.
" e	"	"	"	" Mg	"	+	13 "	"	Good plants and pustules.
" f	"	"	"	" P ₂ O ₅	"	+	13 "	"	Plants elongated and pale; only one minute and poor pustule.
" g	"	"	"	Water only.....	June 24	+	12 "	22 days	Fair plants; one pustule; several flecks.
" h	"	"	"	Normal Detmer	"	+	12 "	"	Fairly normal plants and pustules.
" i	"	"	"	Minus Os	"	+	12 "	"	Pustules small; some leaves shrivelled.
" k	"	"	"	" K	"	—	"	"	Much shrivelled; no pustules.
" l	"	"	"	" Mg	"	—	"	"	Pale and with shrivelled tracts.
" m	"	"	"	" P ₂ O ₅	"	—	"	"	Pale, drawn, and shrivelling.

These results also accord with the appearance of the plants themselves. The want of P_2O_5 results in a long limp growth, and very pale colour, and the attacked spots show corrosion and collapse instead of normal flecks.

Similarly, in plants deprived of K we have pale collapsing patches in place of normal flecks, though the plant is not drawn and etiolated as in the previous case. Where Mg was wanting the drawing and paleness of the leaves were not so marked, and the fungus flecks in the positive case were more like the normal.

That a want of supplied Ca should have very little effect on either host or parasite, was no doubt due to there being small quantities available in the soil and pots. The flecks and pustules were apparently normal, though small.

It is not easy to see why the pots with water only yielded plants with flecks as good as, or even better than those with normal solution, unless the concentration of the salts affects the fungus. However it is probably not to be inferred that in any of these cultures the grass was totally deprived of the given salt—it would get traces from the pots and coir in any case—but the effects of deprivation of salts ought certainly to be felt, and I think were so to some extent in lengthening the incubation period.

So far as the results go they suggest that differences in the supply of minerals affects the development of mycelium and of spores owing to effect on the host—in cultivating the host we also cultivate the parasite.

But I would insist that these trials with special mineral-supplies are merely preliminary. The task in hand was far too extensive for the problems to be solved in one season, and I contemplate carrying the whole matter—or persuading some one to do so—much farther in the future. Meanwhile, the results give suggestions for further work, and much remains to be done along the lines I have here laid down, as well as in directions yet to be opened out.

“On the Physics and Physiology of the Protoplasmic Streaming in Plants.” By ALFRED J. EWART, B.Sc. (Oxon.), D.Sc. (Lond.), Ph.D., F.L.S., Lecturer on Botany in the Birmingham Technical Institute. Communicated by FRANCIS GOTCH, D.Sc., F.R.S. Received January 17,—Read February 20, 1902.

(Abstract.)

The results of an extended series of observations upon this phenomenon have led the author to conclusions which may be summarised as follows:—

The energy of movement is generated in the moving layers themselves, and these are retarded by friction against the non-moving ectoplasm to an extent determined by their own viscosity, and to a much less extent by friction against the cell-sap which is passively carried with the stream. The motor-mechanism is such that no backward reaction is exercised upon the external or internal layers. The velocity of streaming is largely dependent upon the viscosity of the protoplasm, and hence also upon the percentage of water in the latter, but the osmotic pressure exercises little or no direct influence upon streaming.

The activity of diosmosis is not necessarily increased by the existence of streaming, but secondarily induced differences of osmotic pressure may be perceptible between streaming and quiescent cells.

Albuminous solutions containing 89—90 per cent. of water have at 18—20° C. a viscosity of from 0.06 to 0.07, with 95 per cent. of water 0.04, with 72 per cent. 0.29 C.G.S. unit.

Gravity exercises little or no influence upon streaming in small cells, and only a very slight indirect action on streaming in large ones. The velocity of floating particles of greater or less density than the plasma may be distinctly affected by gravity, a fact which indicates that the viscosity of the streaming plasma is comparatively low.

As the temperature rises within certain limits (0° C. to 45 or 50° C.), the viscosity of the plasma decreases, and a large part of the increased velocity is due to this cause alone.

Assuming that the viscosity of the endoplasm corresponds approximately to that of 10 per cent. albumin, forces of 8.75 and 219 dynes would be required to impart velocities of 2 mm. and 0.4 mm. per minute to a gramme of moving liquid in cells of 0.1 and 0.01 cm. internal diameter. The amount of work done in a year represents, in the first case, a theoretical consumption of only $\frac{1}{200000}$ th of a gramme of cane-sugar per gramme of moving liquid, and in the second represents only $\frac{1}{100000}$ th of the energy of respiration, even if 99 per cent. of the energy directed towards streaming is wasted. The force required varies inversely as the square of the radius, so that in a 50-cm. tube of the cribral system of *Cucurbita*, having 2000 sieve-plates with sieve-pores of 2 μ diameter and 10 μ length, a pressure of approximately half an atmosphere would be required to produce a movement in mass of the contents through the tube at a rate of 0.5 mm. per minute. Hence an internal pressure of 5 atmospheres would cause an escape of the contents of an opened sieve-tube at an average rate of about 5 mm. per minute, so long as the pores remained unblocked, which corresponds with the slow exudation taking place when a moderately turgid sieve-tube is first opened.

Regular protoplasmic streaming does not seem to occur even in the individual segments of sieve-tubes, and hence there is no need to discuss whether the protoplasm could generate the propulsive force

required to produce moderately rapid streaming through the pores. The differences of hydrostatic pressure between the different segments of an intact sieve-tube would, however, frequently suffice to produce a direct transference in mass of the fluid contents through the pores of sieve-tubes at a millimetre or so per minute,* for, as evidenced by the influence of gravity and of centrifugal force upon suspended particles, the viscosity of the contents of the sieve-tubes is relatively low.

In the case of bacterium cells of 2 to 0.5 μ diameter, it is very doubtful whether the protoplasm could with economy generate the required propulsive force for perceptible internal streaming movements. The latter take place in diatoms mainly on the external surface, and require relatively little expenditure of energy.

In the case of protoplasmic threads passing through fixed channels in the cell-wall, if these are $\frac{1}{10}$ μ diameter and 5 μ length, a pressure of over 6 atmospheres would be required to drive a liquid of viscosity 0.075 through a single thread at a velocity of 1 mm. per second. So that if a tissue-cell were isolated, it would at the lowest computation take several years for the escape of 1 cubic mm. of the cell-contents through one such thread under a maximal internal osmotic pressure, even supposing that the thread did not become plugged by floating particles and that no cellulose was formed across its exposed end. The outer layers of the ectoplasm appear, moreover, to be very much more viscous than the endoplasm, and hence it is obvious that the threads do not subserve purposes of translocation, although minute particles of protoplasm may be bodily transferred from one cell to another in the course of time.

The direction of streaming is mainly determined by internal factors, and in rotating cells a reversal is only possible in certain cases and under very special conditions.

Changes occur spontaneously however in cells exhibiting circulation. The total resistance during circulation is greater than during rotation, and hence, unless the velocity increases considerably, a change from the former to the latter after stimulation is not due to an increased energy of streaming but to a change in the configuration of the protoplasm.

The energy for streaming can be derived either from aerobic or anaerobic metabolism. Certain species of *Chara* and *Nitella* are in fact facultative anaerobes, and may exhibit slow streaming for 6—8 weeks in the entire absence of free oxygen.

No special chemical changes are connected with streaming.

* According to de Bary ('Compt. Anat.', 1884, p. 177) finger-like processes from the adjoining segments of a sieve-tube meet in the sieve-pores, but remain distinct. This discontinuity has only been observed in dead sieve-tubes, and it probably results from the breaking of the viscous protoplasmic threads at their thinnest points on death, which is a surface-tension effect commonly produced in dying protoplasmic threads.

Of the different constituents of the cell, cellulose, albumin, and chlorophyll are paramagnetic; starch, sugar, oil, water, and probably myosin also, are diamagnetic. Plant-cells usually, though not always, place their long axes parallel to the lines of force in a magnetic field.

The strongest magnetic field used exercised little or no direct effect on streaming, although a pronounced secondary effect is produced on prolonged exposure as the result of inductive action.

The connection between certain forms of streaming movement, such as occur in a few fungi, and metabolism is a wholly indirect one, but this can hardly be a general rule. Indirect relationships exist between streaming, growth, and assimilation, but no direct ones. Similarly the nucleus exercises no direct but a pronounced indirect influence on streaming.

An organised arrangement of the emulsionised protoplasmic particles is probably an essential condition for regular continuous streaming. A great variety of agencies when suddenly applied seem to disturb this arrangement momentarily, and hence produce a temporary cessation of streaming. This shock-effect results from sudden changes of concentration, sudden falls or rises of temperature, momentary electrical excitation, and the sudden application of various poisons.

The minimal, optimal, and maximal temperatures for streaming vary according to the plant or cell examined, and also depend upon (1) the age or condition, (2) the external medium, (3) the duration of the exposure, (4) the supply of oxygen, (5) the rapidity with which the temperature is raised or lowered.

At temperatures above 30° C. the velocity immediately assumed is, in the absence of a shock-effect, always greater than that shown a few hours or a few minutes afterwards. Between 10° and 30° C. the permanent velocity is almost immediately assumed. Below 10° C. the acceleration due to a rise of temperature frequently does not become fully manifest until after a certain lapse of time.

In the case of facultative anaerobes, the response to changes of temperature is less pronounced in the absence of oxygen than in its presence. With short exposures the optimum and maximum points are raised, but with prolonged ones the maximum temperature is lowered by the absence of oxygen.

Strong light retards streaming, while weak light may indirectly accelerate it in chlorophyllous cells. It is still doubtful whether streaming is affected by directly impinging electro-magnetic wave-vibrations other than those of light.

Mechanical disturbances may act as inhibitory stimuli, and may be propagated internally in the form of pressure-waves.

Food-materials exercise both a direct and an indirect effect upon streaming. Acids, alkalies, and metallic poisons all retard streaming, and may cause a temporary shock-stoppage when suddenly applied.

Alcohols and anaesthetics when dilute may accelerate streaming, but when more concentrated always retard it. Alkaloids, which are strong nerve or muscle poisons, have relatively little action upon the streaming protoplasm of plants.

Weak electrical currents may accelerate streaming, strong ones always retard it, while sudden shocks produce a temporary cessation. The latent period of recovery decreases as the temperature rises up to the optimum. Weak constant currents lower the optimal and maximal temperatures for streaming. Cells are more sensitive to electrical stimuli at moderately high temperatures than at very low or very high ones, and the nucleus is fatally affected before the cytoplasm.

The electrical conductivity of the protoplasm undergoes a slight temporary increase on death, and it differs in the living cell from that of the cell-sap and of the cell-wall. The effect produced by a weak constant current bears no relation to its direction with regard to the plane of streaming.

There is little or no analogy between a shock-stoppage of streaming and a muscular contraction, or between a nerve-muscle preparation and a streaming cell. No permanently differentiated nervous mechanism exists in plants, although temporary better-conducting channels may appear as the result of prolonged stimulation or at certain stages in the development of growing organs. Excitatory changes may be transmitted in the protoplasm of a cell of *Chara* or *Nitella* at from 1 to 8 or 20 mm. per second (18° C.), but the rate of propagation from cell to cell in tissues varies from 0.1 to 2 mm. per minute at 18° C. Neither the chloroplastids nor any of the visible floating particles in the protoplasm have any power of independent movement, but are passively carried with the moving stream.

The only kind of energy which appears capable of producing streaming movements under the conditions existing in plant-cells is surface-tension energy, and this is probably brought into play by the action of electrical currents which traverse the moving layers, and are maintained by chemical action in the substance of the protoplasm. These currents may act upon more or less regularly arranged bipolar particles of emulsionised protoplasm in such a manner as to reduce their surface-tension on the anterior side, or increase it on the posterior one, hence producing streaming movement in a definite direction.

"Preliminary Note on a Method of Calculating Solubilities, and the Equilibrium Constants of Chemical Reactions, and on a Formula for the Latent Heats of Vaporisation." By ALEXANDER FINDLAY, M.A., B.Sc., Ph.D. Communicated by Professor RAMSAY, F.R.S. Received February 1,—Read February 13, 1902.

I. A number of years ago, it was shown by Ramsay and Young,* that for any pair of closely related substances, the ratio of the absolute temperatures corresponding to equal vapour pressures is constant, *i.e.*, $T_1/T'_1 = T_2/T'_2$. In the case of substances not closely related, it was shown, generally, that the relation between the absolute temperatures at which the two substances have equal vapour pressures could, with very great approximation, be expressed by the equation, $R = R' + c(t' - t)$, R being the ratio of the absolute temperatures, T/T' or T'/T , at which the substances both have any given vapour pressure; c is a constant which may be 0 or a small positive or negative number; and t' and t are the temperatures of one of the substances corresponding to these two vapour pressures. Or, since the variation of the ratio R from constancy was found to be practically a linear function of the temperature, we may also express the above relationship by means of the equation $T/T' = a + bT$. In this equation a is of the order unity, and b of the order 0.001 to 0.0001, and $-b$ is equal to $+c$ of the previous equation. It was further shown by Ramsay and Young that by means of the above equation, if the vapour pressure curve of one substance is known, the vapour pressure curve of any other substance can be calculated from the values, at any two temperatures, of the vapour pressure of that substance. The above relationship was tested by Ramsay and Young in the case of 23 pairs of substances, and has also been found to hold up to the critical pressure.†

The degree of success with which the method can be used, is shown by Table I, which I take from the memoir of Ramsay and Young above mentioned.

At the suggestion of Professor Ramsay, I undertook the investigation of whether any similar method could be obtained for the calculation of solubilities; and I have found that a precisely similar equation to that of Ramsay and Young connects the absolute temperatures at which two substances have equal solubilities; that, therefore, the equation $R = R' + c(t' - t)$ holds good with respect to the *solubilities* of any two substances. In this equation R and R' now denote the ratios of the absolute temperatures at which the substances have equal solubili-

* 'Phil. Mag.,' 1886, vol. 21, p. 33.

† In this way the vapour pressures of argon, krypton, and xenon have been calculated by Ramsay and Travers ('Phil. Trans.,' A, vol. 197, 1901, p. 47).

Table I.—Calculation of the Vapour Pressures of Carbon Disulphide from the Vapour Pressures of Water.

$$c = 0.0006568.$$

Pressures in mm.	Abs. temps. of water.	Ratios of abs. temps. of CS ₂ to those of H ₂ O.	Smoothed ratios.	Re-calculated abs. temps. of CS ₂ .	Observed abs. temps. of CS ₂ .
50	311.3	0.8161	0.8160	254.0	254.05
100	324.7	0.8245	0.8242	267.6	267.7
150	333.1	0.8301	0.8296	276.3	276.5
200	339.6	0.8339	0.8338	283.2	283.2
300	348.9	0.8403	0.8400	293.1	293.2
400	356.0	0.8448	0.8449	300.8	300.75
500	361.7	0.8485	0.8483	306.8	306.9
600	366.5	0.8517	0.8519	312.2	312.15
700	370.7	0.8545	0.8545	316.8	316.75
800	374.45	0.8567	0.8571	320.0	320.8
900	377.8	0.8589	0.8590	324.5	324.5
1000	380.85	0.8612	0.8611	327.95	328.0
1500	393.2	0.8695	0.8692	341.8	341.9
2000	402.5	0.8753	0.8757	352.5	352.3
3000	416.5	0.8852	0.8850	368.6	368.7
5000	435.85	0.8967	0.8978	391.3	391.7

ties; c is a constant which may, possibly, have the value 0, but which, in all the cases I have examined, has a small positive or negative value: t' and t are the temperatures at which one of the substances has the two values of the solubility in question. *The above equation also holds no matter whether the substances are ionised or are non-ionised, or whether their heat of solution is positive or negative.*

A method, which is in all points analogous to that employed by Ramsay and Young* for the calculation of vapour pressures, can thus be made use of for the calculation of solubilities. In order to calculate the solubility of any substance B by means of the known values of the solubility of another substance A, one proceeds as follows:—The solubility of B at any two absolute temperatures T'_1 and T'_2 is determined. On dividing these temperatures into the temperatures T_1 and T_2 , at which A has the same solubility, the ratios T_1/T'_1 and T_2/T'_2 are obtained. These ratios are now plotted as abscissæ against the corresponding temperatures of the substance A as ordinates, and a straight line drawn through the two points thus obtained. From this straight line curve, now, different ratios can be read off, and also the corresponding values of the absolute temperatures of substance A. By dividing the absolute temperature T of substance A by the corresponding value of the temperature ratio, the

* Loc. cit.

temperature T' is obtained, at which the solubility of B is equal to that of A at the temperature T . In this way the solubilities of B covering the range of known solubilities of A can be calculated.

I have tested this method in the case of a considerable number of substances of very varied character and solubility, and have, in all cases, found a very satisfactory agreement between the values of the solubility as calculated by means of the above method and those determined experimentally. This is readily apparent from Tables II, III, IV, and V.

In the following tables the solubilities are expressed in gramme-molecules of substance in 100 gramme-molecules of solvent (water). With regard to the solubilities of zinc chloride and bromide, and of cadmium bromide, the values obtained by R. Dietz* were employed. In the case of the other salts, the solubilities of potassium chloride and bromide were determined by de Coppet;† those of potassium nitrate by Andreä,‡ and those of the oxy-benzoic acids by Walker and Wood.§ The values of R , or the ratio T/T' are calculated by means of the temperatures marked with an asterisk.

Table II.— ZnBr_2 and $\text{ZnCl}_2 \cdot \text{H}_2\text{O}$.

$$c = 0.00316.$$

Solubility observed.	Solubility of 2 calculated.	Abs. temp. for 1 observed.	Abs. temp. for 2 observed.	Abs. temp. for 2 calculated.	Ratio of tempera- tures.
46.9	47.2	308.0	283.2	283.6	1.086
47.4	—	313.0*	283.9*	—	1.102
48.0	48.0	319.2	284.5	284.4	1.122
49.5	49.5	333.0	285.7	285.7	1.165
50.0	—	338.0*	286.2*	—	1.181
51.5	51.2	352.0	287.6	287.3	1.225
52.5	51.8	360.7	288.4	287.8	1.253
53.8	52.9	372.5	289.4	288.7	1.290

As can be seen from the above table, the solubility of the zinc bromide changes much less rapidly with the temperature than that of zinc chloride. Since the solubility curve of zinc chloride is rather steep, a comparatively small error in the temperature gives a comparatively large error in the calculated value of the solubility. The agreement, nevertheless, between the calculated and the determined values of the solubility is very satisfactory.

* 'Wissenschaftliche Abhandlungen der Physikalisch-technischen Reichsanstalt'; and 'Berichte D. Chem. G.', 1899, vol. 32, p. 90.

† 'Ann. de Chimie' [5], vol. 30, p. 414.

‡ 'Jl. Prakt. Chem.', vol. 137, p. 474.

§ 'Trans. Chem. Soc.', 1898, vol. 73, p. 618.

Table III.—KBr and KCl.

$$c = -0.0001488.$$

7.0	6.9	259.5	277.2	276.9	0.9370
8.0	—	272.6*	291.5*	—	0.9351
9.0	9.1	295.5	305.5	305.9	0.9331
10.0	10.0	297.8	319.7	319.7	0.9313
11.0	10.9	310.5	334.1	334.0	0.9294
12.0	—	323.0*	348.2*	—	0.9276
13.0	12.9	335.5	362.5	362.4	0.9257
14.0	13.85	347.7	377.1	376.3	0.9239

Table IV.— $\text{CdBr}_2 \cdot 4\text{H}_2\text{O}$ and KNO_3 .

$$c = 0.00148.$$

4.2	4.0	274.0	289.5	288.6	0.9494
4.6	—	278.0*	291.4*	—	0.9553
5.8	6.0	288.0	296.0	296.8	0.9701
6.6	6.87	293.0	298.7	299.7	0.9775
7.6	7.8	298.0	302.0	302.5	0.9849
8.7	—	303.0*	305.3*	—	0.9924
10.0	9.61	308.0	309.3	308.0	0.9998
11.4	10.60	313.0	313.0	310.8	1.007

The very satisfactory agreement found in the case of the above salts is of interest from the fact that the two substances are ionised in a very different degree.

Table V.—*m*- and *p*-Oxybenzoic Acids.

$$c = 0.00104.$$

0.088	0.0868	286.8	300.0	300.7	0.9537
0.096	0.0976	288.7	301.7	302.1	0.9556
0.108	—	291.5*	304.1*	—	0.9546
0.116	0.114	293.2	305.6	305.3	0.9603
0.124	0.124	295.1	306.6	306.6	0.9623
0.136	0.1396	297.5	308.0	303.3	0.9648
0.148	0.1528	299.5	309.2	309.7	0.9669
0.172	0.1756	303.1	311.8	312.2	0.9706
0.190	—	305.0*	313.6*	—	0.9726
0.210	0.202	307.0	316.0	315.0	0.9746
0.220	0.206	307.9	317.1	315.6	0.9756
0.240	0.2156	309.8	318.9	316.9	0.9775

The preceding examples are only a few of the cases in which I have tested the validity of the relationship above stated, and, as can be seen, the agreement between theory and experiment is very satis-

factory. An equally good agreement was also shown in the other cases which I investigated. From the considerable number of cases I have studied, it would appear that the above method can be applied to the calculation of solubilities with close approximation to the truth. As to the range of temperatures or solubilities over which the relationship holds good, no definite statement can be made as yet. In almost all cases the relationship was tested over the whole range of available data; that is to say, over the range of temperatures within which the two substances under comparison have equal solubilities. Several of the deviations are no doubt due to inaccuracies in the determination of the solubilities, and to errors in the drawing of the curves or reading from them.

II. *Calculation of Equilibrium Constants.*—The formula of Ramsay and Young, further, can be applied not only, as has already been shown, to the calculation of vapour pressures and of solubilities, but it can also be used for the purpose of calculating the equilibrium constants of chemical reactions. In this case, R and R' denote the ratios of the absolute temperatures at which the values of the equilibrium constant of the two reactions are equal. In this case, also, if we know the temperature curve of the equilibrium constant of one reaction, it will be possible to calculate the temperature curve of the equilibrium constant of another reaction, by determining the value of that constant at two temperatures. This is shown by the figures given in Table VI. The two reactions which were compared were those represented by the equations $2\text{HI} \rightleftharpoons \text{H}_2 + \text{I}_2$,* and $2\text{CH}_3 \cdot \text{CO} \cdot \text{CH}_3 \rightleftharpoons \text{CH}_3 \cdot \text{CO} \cdot \text{CH}_2 \cdot \text{C}(\text{CH}_3)_2\text{OH}$,† two reactions, therefore, which are of a most dissimilar character. As the values of the equilibrium constant of these two reactions were not determined at a sufficient number of suitable temperatures for the present purpose, it was necessary to calculate the values of the constant at other temperatures. In the case of hydriodic acid, the constant was calculated for the temperatures 520° , 530° , 540° , 550° , 560° , by means of the formula given by Bodenstein:‡

$$\log_e K = \frac{90 \cdot 48}{T} - 1 \cdot 5959 \log_e T + 0 \cdot 0055454 T + 2 \cdot 6981.$$

The values found were:—

t	520°	530°	540°	550°	560°
K	0·02563	0·02658	0·02759	0·02864	0·02974

In the case of the condensation of acetone to diacetone alcohol, in which case the equilibrium constant was determined at only two

* Bodenstein, 'Zeitschr. Physik. Chem.,' 1899, vol. 29, p. 293.

† Koelichen, 'Zeitschr. Physik. Chem.,' 1900, vol. 33, p. 123.

‡ *Loc. cit.*

temperatures, viz., 0° and 25·2°, the values of the constant were calculated at 10°, 40°, 60°, 80°, 100°, by means of the integrated form of the van't Hoff equation :

$$\log_e K_1 - \log_e K_2 = \frac{q}{R} \left(\frac{1}{T_2} - \frac{1}{T_1} \right)$$

the value of q , the heat of reaction, being regarded as constant and equal to - 77·4 centuple calories.* R is the "gas constant." The error introduced here can scarcely be very large.

Table VI.

Equilibrium constant.	Equilib. constant for 2, calc.	Absol. temp. for 1 observed.	Absol. temp. for 2 observed.	Absol. temp. for 2 calc. $c = 0\cdot0114$.	Ratio of temperatures.
0·0266	0·0265	805·2	379°·0	380°·4	2·1162
0·0270	—	809·5*	374·0*	—	2·1644
0·0275	0·0275	814·0	368·0	367·5	2·2148
0·0280	—	818·0*	362·0*	—	2·2596
0·0285	0·0283	821·5	356·0	357·3	2·2988
0·0290	0·0285	826·0	350·0	351·6	2·3492
0·0295	0·0292	829·0	344·0	347·9	2·3928

As can be seen from the above figures, the agreement between the calculated and determined values of the equilibrium constants is exceedingly good, especially when one considers how different the two reactions are in character and also that in one case the equilibrium constant increases, in the other case, diminishes with the temperature.

So far, this is the only case in which I have been able to test the application of the formula $R = R' + c (t' - t)$ to the calculation of equilibrium constants. On account, however, of the analogy existing between the change of the vapour pressure, solubility, and equilibrium constant, with the temperature, it may be confidently expected that when other cases come to be tried, confirmation of the relationship will be obtained.

On account of the practical importance of the above relationships, I have thought it well, in this preliminary note, to publish them in the present empirical form. I hope, however, at a later time, to discuss the question in greater detail, with the help of a larger number of examples, and also to examine the subject from the theoretical standpoint. It may, however, be remarked here, that the whole work has been based on the form of the thermodynamic equation given by

van't Hoff, $\frac{d \log_e x}{dt} = \frac{Q}{RT^2}$, an equation which at once shows the

* Koelichen, loc. cit.

analogies existing between vapour pressures, solubilities, and equilibrium constants of chemical reactions.

III. *Empirical Formula for Latent Heats of Vaporisation.*—During the progress of the preceding work, I was led by a consideration of certain empirical formulæ put forward a number of years ago by W. C. Unwin,* connecting the pressure, temperature, and volume of saturated vapours, to a formula by means of which from the latent heats of vaporisation of one substance the values of the latent heats of vaporisation of another substance may be calculated. If L_1 is the known latent heat at the absolute temperature T_1 of the first substance, and L_2 the latent heat of the second substance at the temperature, T_2 , at which the vapour pressure of the second substance is equal to that of the first substance at the temperature T_1 , then $L_1 = L_2 T_2^x$, where x is a constant. A formula which is derived with greater strictness, but which contains two constants, is $L_1/L_2 = T_1^{x1}/T_2^{x2}$. In some cases the latter formula gives somewhat better values than the former, but for most purposes the simpler formula may be employed. These formulæ appear to be suitable for calculating the latent heats of vaporisation at not too high pressures, but break down in some cases at pressures over 10,000 mm., although in other cases they hold even at pressures of over 20,000 mm.

In Tables VII and VIII are contained some of the results which have been obtained using the simpler formula.

Table VII.—Benzene and Methyl Acetate. $x = -0.008847$.

Pressure in mm.	Absol. temp. of benzene.	Absol. temp. of methyl acetate.	Heat of vaporisation of benzene.	Heat of vaporisation of methyl acetate.	
				Deterd.	Calc.
			cal.	cal.	cal.
1335	373	347.3	92.06	96.95	—
4335	423	390.9	81.01	83.51	85.40
10650	473	433.4	69.14	70.95	72.95
22182	523	475.0	49.71	52.28	52.49

As can be seen, the agreement between the determined and calculated values of the latent heat of vaporisation, is very good. In some cases, however, which I have investigated, *e.g.*, in the case of benzene and hexane, the agreement is not quite so good.

The values of the latent heats of vaporisation of benzene, methyl acetate, and ethyl alcohol, given in the fourth and fifth columns of the

* 'Phil. Mag.,' 1886, vol. 21, p. 299.

Table VIII.—Benzene and Ethyl Alcohol. $\alpha = -0.1297$.

Pressure in mm.	Absol. temp. of benzene.	Absol. temp. of ethyl alcohol.	Heat of vaporisation of benzene.	Heat of vaporisation of ethyl alcohol.	
				Deterd.	Calc.
26.54	273	255.0	cal. 106.17	cal. 221.0	—
268.3	323	327.4	99.72	214.96	211.3
1335	373	366.4	92.06	200.2	198.0
4335	423	403.3	81.01	177.5	176.4
10650	473	438.5	69.14	152.7	152.2
22182	523	473.2	49.71	116.6	110.5

Tables VII and VIII, were calculated by means of the thermodynamic equation

$$\frac{L}{s_1 - s} = \frac{T}{J} \cdot \frac{dp}{dt},$$

using the data of Ramsay and Young* for ethyl alcohol; of Young† for benzene; and Young and Thomas‡ for methyl acetate.

It is probable that, in some cases at least, a formula similar to that given above may be used for the calculation of heats of solution and of chemical reaction, but at present the data necessary for testing this are not available.

Summary.

1. It is shown that by means of the equation $R = R' + r(t' - t)$ (*vide* p. 471), vapour pressures (Ramsay and Young), solubilities, and the equilibrium constants of chemical reactions can be calculated.

2. The latent heat of vaporisation of a substance can be calculated from the latent heat of vaporisation of another substance, within a certain range, by means of the empirical equation $L_1 = L_2 T_2^\alpha$.

My thanks are due to Professor Ramsay for the advice which he has given during the progress of the present work.

* 'Phil. Trans.,' 1886, Part I, p. 71.

† 'Trans. Chem. Soc.,' 1891, vol. 59, p. 134.

‡ *Ibid.*, 1893, vol. 63, p. 1210.

"Note on the Anomalous Dispersion of Sodium Vapour." By
W. H. JULIUS, Professor of Physics in the University of
Utrecht. Communicated by C. VERNON BOYS, F.R.S. Re-
ceived February 17,—Read February 20, 1902.

The importance of the study of anomalous dispersion in gases, both for testing dispersion theories and for investigating the connection of the subject with solar physics,* has induced Professor R. W. Wood to publish a highly interesting paper on the anomalous dispersion of sodium vapour.†

In most of his experiments, Wood deals with a much denser vapour than I had studied; this may be the reason that he believes he finds a difference between his views and mine about a point on which, in fact, we do agree.

On p. 160,‡ Wood describes the spectrum of electric light that has passed through a tube containing sodium vapour of increasing density, and how in a few seconds the vapour becomes so dense that total absorption of all the light between the D-lines occurs. Then he says: "Julius expresses the opinion that this disappearance of the light between the lines is only a result of the strong dispersion"; and he is of a different opinion. This, however, proves, I fear, that he really did not read my paper rightly, as he suspected might be the case (p. 169).§

Indeed, speaking of the difference in the result, obtained by Becquerel and by myself in studying the dispersion caused by a sodium flame, I suggested|| that "perhaps Becquerel's flame contained more sodium than mine"; for I felt sure, that in a sufficiently dense vapour the absorption would extend over a broader region of the spectrum. Some lines further, alluding to the absence, in the spectrum, of the light that is strongly deflected by anomalous dispersion and thus falls outside the spectroscopie, I said: "Here, then, we have a case where the absorption spectrum of a vapour exhibits broad bands not deserving the name of absorption bands"; and then . . . "It would be worth while investigating in how far the anomalous dispersion can have influenced cases, in which broadening or reversal of absorption-lines have been observed."

So it is quite clear that I did not deny the possibility of getting

* W. H. Julius, "Solar Phenomena considered in connection with Anomalous Dispersion of Light," *Roy. Acad. Amsterdam Proc.*, vol. 2, p. 575; *Astron. Nachr.*, 3672.

† R. W. Wood, "The Anomalous Dispersion of Sodium Vapour," *Roy. Soc. Proc.*, vol. 69, p. 157.

‡ *Loc. cit.*

§ *Loc. cit.*, p. 169.

|| *Roy. Acad. Amsterdam Proc.*, p. 578.

broad bands by absorption. I have never expressed the opinion, that the disappearance of the light between the D-lines in the absorption spectrum of dense sodium vapour is only a result of the strong dispersion; but I warned against always ascribing the observed dark bands to absorption only.

Wood's recent researches are very important as a contribution to our knowledge of dispersion in general. For the present their bearing on the spectral phenomena exhibited by the light from the chromosphere and from sun-spots, seems not to be so direct, because, most probably, the density of the vapours is much less in the solar atmosphere than in the dispersion tubes used by Wood in his brilliant experiments.

"On the Increase of Electrical Resistivity caused by Alloying Iron with Various Elements, and the Specific Heat of those Elements." By W. F. BARRETT, F.R.S., Professor of Experimental Physics in the Royal College of Science for Ireland. Received December 16, 1901—Read February 6, 1902.

In the following note I wish to draw attention to a connection which appears to exist between the electric conductivity of certain alloys of iron and the specific heats, and hence atomic masses, of the particular elements with which the iron is alloyed. In a paper published in the 'Transactions' of the Royal Dublin Society the electric conductivity and magnetic permeability of a very large and, I believe, unique collection of alloys of iron is given.* These alloys, it may be mentioned, have been prepared with great care by my friend, Mr. R. A. Hadfield, Managing Director of the Hecla Steel Works, Sheffield. Of the alloys made, 110 different specimens were found homogeneous, and could be forged and rolled; these were analysed at the Hecla works, and submitted to similar heat treatment—all being carefully annealed under the direction of Mr. Hadfield.

The specimens were in the form of rods, of nearly circular cross-section, about one-half a centimetre in diameter and 104 cms. long. The conductivities were found by the potential method, a standard of pure copper being employed. Although the determination of the mean sectional area of the specimens was made with great care, by numerous measurements of each rod with a micrometer screw, and also by means of water displacement, yet, owing to slight irregularities in the diameters of the rods, and the numerical importance of this value in

* "On the Electrical Conductivity and Magnetic Permeability of Various Alloys of Iron," by W. F. Barrett, F.R.S., W. Brown, B.Sc., and R. A. Hadfield, *M.Inst. C.E.*, 'Trans. Royal Dublin Society,' January, 1900.

estimating the conductivity, the results obtained can only be regarded as a more or less close approximation to the true value.*

A more formidable difficulty is the impossibility of obtaining such a collection of alloys free from the admixture of disturbing impurities. Slight variations in the amount of these impurities, especially in the amount of carbon present, produce in the low percentages a profound effect on the conductivity and also on other physical properties of these alloys. However, in most of the specimens Mr. Hadfield has succeeded in reducing these impurities to a lower amount than any other large collection of alloys yet made. Carbon, manganese, and silicon were the impurities most commonly present, and the sum of these in the specimens selected was, as a rule, under 1 per cent.

There were sixty-eight specimens of these alloys having various percentages of a single element added to iron; these were as follows (the element with which the iron is alloyed being named in the first column):—

Manganese....	18	specimens ranging from	$\frac{1}{2}$ to $18\frac{1}{2}$	per cent. of Mn.	
Carbon	13	" "	$\frac{1}{10}$ " $1\frac{1}{2}$	"	C.
Nickel	12	" "	$\frac{1}{2}$ " 31	"	Ni.
Tungsten	4	" "	1 " $15\frac{1}{2}$	"	W.
Chromium....	3	" "	2 " $9\frac{1}{2}$	"	Cr.
Copper.....	3	" "	$1\frac{1}{2}$ " 3	"	Cu.
Aluminium...	3	" "	$\frac{1}{2}$ " $2\frac{1}{2}$	"	Al.
Silicon	2	" "	$2\frac{1}{2}$ " $5\frac{1}{2}$	"	Si.

In addition to these there were fifty-two other specimens having two or more of the above elements alloyed to iron in various proportions.

In every case, with the doubtful exception of copper,† a decrease in conductivity was found to result from alloying iron with another metal, even when that element, as in the case of aluminium, had itself a conductivity far higher than that of the iron. From the conductivities of the specimens, their specific resistances were calculated; these were plotted against the percentages of the added element. A series of fairly smooth curves were thus obtained for each alloy. These curves are shown in fig. 1. It will be noticed that the addition of silicon or aluminium to iron produces the greatest increase in electric resistance and tungsten the least, and that a remarkable change in the electric resistivity of nickel steels occurs at high percentages. There is obviously no connection between these curves and the electric conduc-

* It was satisfactory, however, to find that in a dozen or more specimens which were drawn into wire, and the specific resistances of which were measured in the ordinary way, the results obtained corroborated the values found from the rods.

† The specimens of copper-iron alloy were very few, and the results masked by the varying impurities they contained; very little alteration of the conductivity appeared to be produced by the addition of copper to iron.

tivity of the added element; *e.g.*, the metal aluminium is a far better conductor than nickel, and yet alloyed with iron it decreases the conductivity of iron far more than nickel. Silicon and aluminium alloys of iron are extremely soft, and we should expect them to be better conductors than the hard tungsten steels, and far better than the

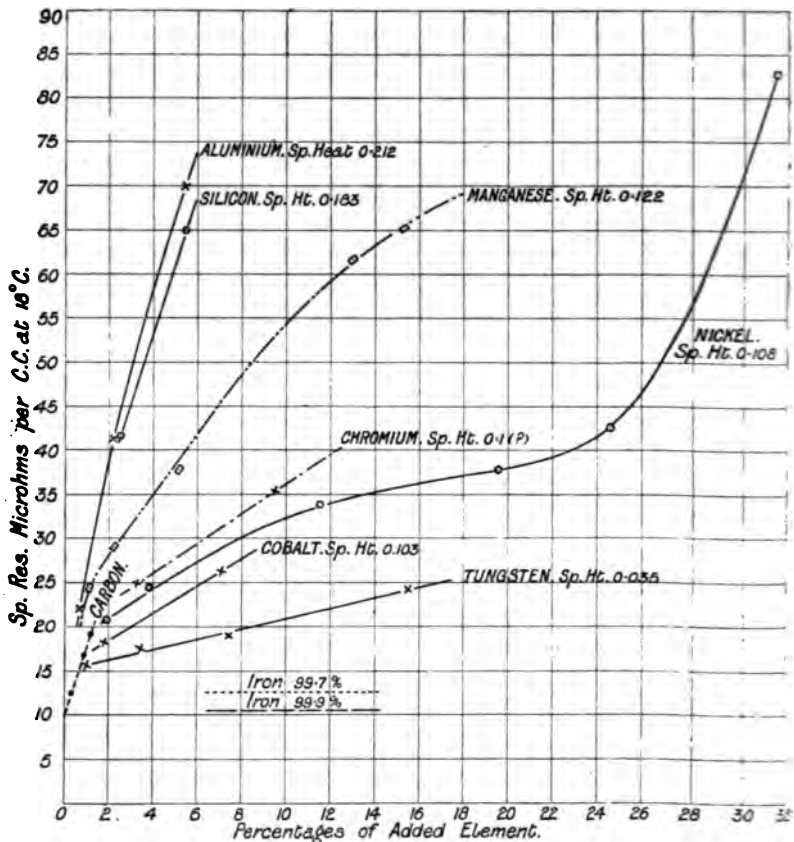


FIG. 1.—Specific Electric Resistance of Alloys of Iron.

manganese steels, which are very hard, but the reverse is the case. Owing to their extreme softness, the alloys of silicon and aluminium with iron have an enormously high magnetic permeability, which will be referred to in the sequel.

The resistance of iron containing 0.1 and 0.3 per cent. of carbon and other impurity is shown by the horizontal dotted lines in fig. 1. From these data the increase of resistance produced by alloying iron with a given percentage of any of the elements named can be determined by

selecting that specimen of iron which has about the same carbon or other impurity as that contained in the particular alloy and subtracting the resistance of the iron. The result is shown in the next table.

Table I.—Approximate Increase in Electric Resistivity (in microhms per c.c. at 18° C.) of Annealed Iron Alloys produced by adding to Iron Different Percentages of the Elements named in the first column.

Alloys of iron with	Percentage of added element.		
	2 per cent.	3 per cent.	5 per cent.
Tungsten	4·0	5·0	6·0 microhms
Nickel.....	7·0	9·0	13·0 „
Chromium	10·0	11·5	14·0 „
Manganese	16·0	18·0	24·5 „
Silicon	26·0	34·0	49·0 „
Aluminium.....	28·0	36·0	54·0 „

Dividing these values by the percentage at the head of each column, it will be seen that as the alloy becomes richer in the added element the increase of electric resistivity becomes less and less for every *one* per cent. of the element added. The comparative effect of carbon is difficult to ascertain owing to various causes, (*a*) the impossibility of making homogeneous alloys of iron with large percentages of carbon, the highest carbon in our specimens being $1\frac{1}{4}$ per cent.; (*b*) the different conditions in which the carbon may exist in the alloy; and (*c*) the difficulty of excluding impurities such as manganese and silicon, minute amounts of which exert a serious effect on the conductivity. However, by comparing the conductivity of alloys of iron having nearly similar quantities of Si and Mn, but containing different quantities of carbon, a close approximation of the effect of this element on the conductivity of iron is arrived at. The result shows that the increase of *one-tenth of one per cent.* of carbon in iron containing 0·03 carbon causes an increase in the specific resistance of nearly 2 microhms per c.c., but when the carbon is raised from 0·13 to 1·13 the increase in specific resistance is at the rate of 5 microhms for 1 per cent. of added carbon, about one-fourth of the rate of increase at the smaller percentages. The position of carbon on Table I therefore appears to lie near to chromium. From specimens of two impure alloys containing cobalt with iron, the increase of resistance produced by cobalt was estimated to be somewhat less than nickel but greater than tungsten; its position in Table I would therefore appear to lie near to nickel.

If now we compare the increase of resistance in iron produced by alloying it with these elements, with the atomic weights of the added elements, we find, as a rule, that those elements with the highest

484 *Increase of Electrical Resistivity caused by Alloying Iron.*

atomic weight produce the *least* increase in electric resistivity and *vice versa*. For the sake of such a comparison, the *molecular weights* of these elements, if we knew them, rather than the atomic weights, should be taken. This is especially true in the case of carbon, the molecular weight of which, if this analogy holds good, would appear to be four times its atomic weight. If, instead of the atomic weights, we take the *specific heats* of these elements, so far as they are known, we are likely to arrive at a better knowledge of their relative atomic or molecular masses, and a comparison of the order of specific heats with the order of increase of electric resistivity is very striking. This is shown in the next table.*

Dividing the increase in electric resistivity by the percentage of the added metal, we obtain the increase in the specific resistance of iron produced by 1 per cent. of the added element. This is shown for a 2 per cent. alloy (except in the case of carbon) in the second column of Table II, along with the specific heats and atomic weights of the elements named in the first column.

Table II.

Alloy of iron.	Increase of resistivity for 1 per cent.	Specific heat.	Atomic weight.
Tungsten	2·0	0·035	184
Cobalt.....	3·0	0·107	59
Nickel.....	3·5	0·109	59
Chromium	5·0	0·1 (?)	52
Carbon†	5·0	0·160‡	12
Manganese	8·0	0·122	55
Silicon.....	13·0	0·183	28
Aluminium.....	14·0	0·212	27

I venture to think that the correspondence shown in columns 2 and 3 of the foregoing table is something more than a chance coincidence. It is, however, desirable to have exact determinations of the resistivity of a larger number of alloys of iron before any definite conclusions can be reached.

A series of experiments are in progress for the measurement of the relative *thermal* conductivity of the foregoing alloys. About forty determinations have been made, and so far the order of thermal

* The values for the specific heats are taken from Landolt and Börnstein's great work on Physical Constants. The specific heat determinations are chiefly by Regnault, between 9° and 97° C.

† For a 1 per cent. alloy.

‡ As *graphite*; as *diamond* the specific heat of carbon is 0·113, according to H. F. Weber.

conductivity has been found to be the same as that of their electrical conductivity.

As regards the *magnetic permeability* of these alloys, the order is very different from that of their electric conductivity. The most highly permeable alloys are those formed of aluminium and silicon with iron. In fact, the magnetic permeability of an alloy of iron with $2\frac{1}{2}$ per cent. of silicon exceeds that of the best and purest annealed iron up to a field of 10 C.G.S. units. Still more remarkable is a pure and well annealed alloy of aluminium and iron; although it contains a considerable percentage of non-magnetic elements, its magnetic permeability and maximum induction up to a field of 60 units exceeds the best and purest annealed iron that I can obtain, a specimen of Swedish charcoal iron containing 99.9 per cent. of iron, all the specimens having been subjected to a precisely similar annealing process.

It is possible the increased magnetic susceptibility given to iron by aluminium, and to a less extent by silicon, may be due to the strong chemical affinity which these elements have for oxygen, whereby any of this gas which might be dissociated in the molten iron would be removed, and the texture of the metal thus rendered closer and more uniform. In the same way, by combining with the oxygen, they would remove, as my colleague Professor Hartley suggests, traces of oxide of iron, more or less diffused through all iron; and the presence of which would certainly lower the magnetic susceptibility.

The remarkable magnetic properties of these two alloys, especially of the aluminium-iron alloy, is a matter not only of considerable theoretic interest but obviously is also of great practical importance in electrical engineering.

“On a Pair of Ciliated Grooves in the Brain of the Ammocete apparently serving to promote the Circulation of the Fluid in the Brain-cavity.” By ARTHUR DENDY, D.Sc., F.L.S., Professor of Biology in the Canterbury College, University of New Zealand. Communicated by Professor G. B. HOWES, LL.D., F.R.S. Received February 7,—Read February 20, 1902.

The peculiar and apparently hitherto undescribed structures which form the subject of the present communication, were first discovered in the course of an as yet unfinished investigation of the parietal organs in the New Zealand Lamprey (*Geotria australis*). The *Ammocete* of this interesting species is known to us only through two specimens:

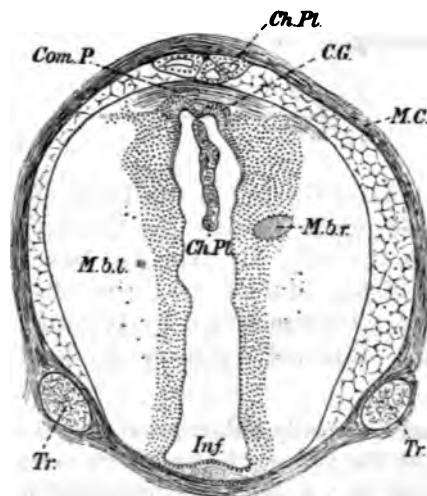
one of these was briefly described by Kner in 1869* ; the other was for many years in the Museum of the Otago University, Dunedin, and was forwarded to me for investigation by the present curator, Professor W. B. Benham, D.Sc., to whom I desire to express my indebtedness for his great kindness.

The specimen which I have thus had the opportunity of investigating was labelled in the handwriting of the late Professor T. J. Parker, F.R.S.—“*Ammocetes* stage of *Geotria*—Opoho Creek. Jany., '84. Chromic and Osmic.” It had evidently been preserved with the utmost care, and proved, in spite of its age, to be in an admirable state for histological investigation.

The total length of the specimen was about 57 mm., and the greatest diameter in the middle 3·5 mm. With a view especially to the study of the parietal organs, the head region was cut into a series of transverse sections by the usual paraffin method, and the sections were stained on the slide ; for staining, carbol-fuchsin proved satisfactory.

On examination, the sections thus prepared showed not only a well-developed parietal eye and the structures usually associated therewith, but also a pair of conspicuous ciliated grooves lying in the roof of the brain in the neighbourhood of the posterior commissure.. These grooves run longitudinally from the recessus sub-pinealis to the hinder margin of the posterior commissure. They are most conspicuous beneath the commissure itself (figs. 1, 2), in which region they are

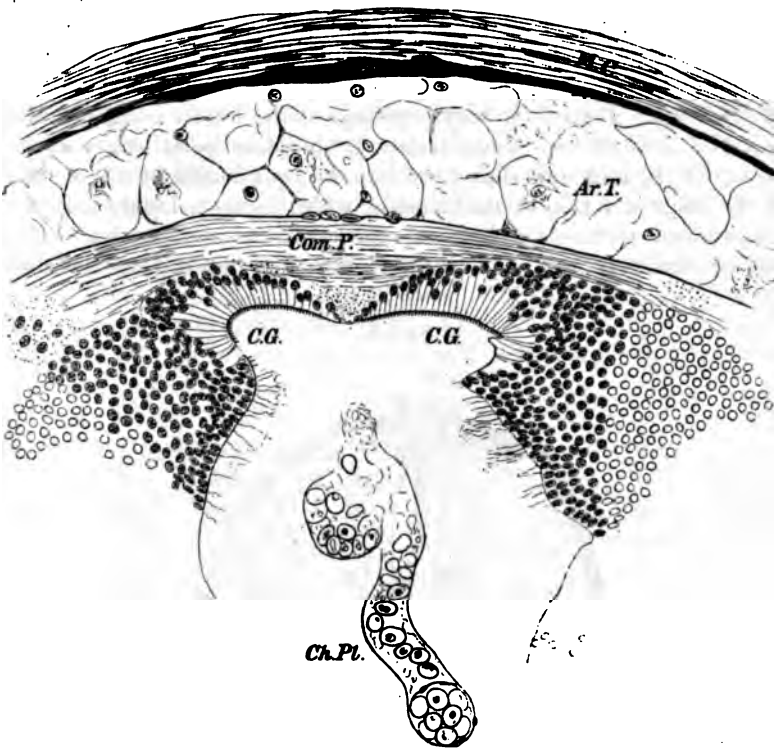
FIG. 1.



* "Reise der Österreichischen Fregatte *Norara* um die Erde," "Zoologie," Bd. 1. *Fische*. p. 421.

lined by a sharply defined epithelium of very long columnar cells, totally different in appearance from the epithelium which lines the remainder of the brain-cavity. The inner margins of the two grooves in this region touch one another in the middle line. Their lumina are deeply concave and open widely into the brain-cavity, which is here represented by a rather narrow vertical slit, terminating below

FIG. 2.

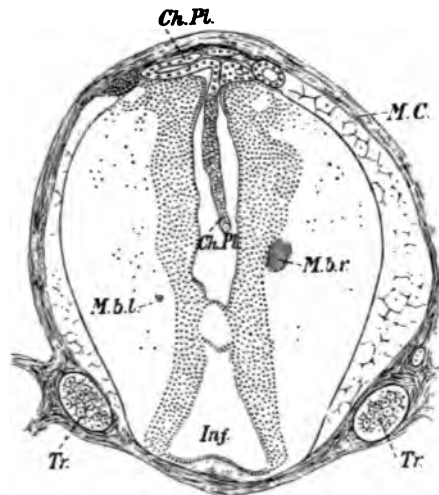


in the infundibulum (fig. 1, Inf.). Thus the cross-section of the two ciliated grooves lying beneath the posterior commissure has the form of the figure ∞. Their lining epithelium, as already pointed out, is conspicuously different from the lining epithelium of the brain-cavity elsewhere. It is composed of narrow columnar cells with conspicuous nuclei (fig. 2). While very short at the margins of the grooves, these cells gradually increase in length towards the middle, so that the lining epithelium is very much thicker in the middle of each groove than it is at the two edges. The inner surface of each groove is covered by a thick coating of very short cilia. The transition from

the epithelium which lines the grooves to that which lines the adjacent parts of the brain-cavity takes place abruptly, and there are indications on the latter of very much longer cilia than occur in the grooves. As far back as 1873, indeed, Langerhans pointed out in his researches on *Petromyzon planeri* that the ventricles of the brain are lined with ciliated cylinder-epithelium.*

The upper and inner part of each groove is in this region imbedded in the substance of the posterior commissure; the lower and outer portion in the mass of cells which forms the inner part of the brain substance (fig. 2). Anteriorly the two grooves diverge from one another on the roof of the recessus sub-pinealis, and disappear in the deep crevices between the ganglia habenulæ and the side walls of the brain. Posteriorly they terminate at the hinder margin of the posterior commissure. Immediately behind this point the choroid plexus of the mid-brain dips down into the aquæductus Sylvii, or iter, in the shape of a thin vertical lamella, which forms a highly vascular longitudinal septum dividing the upper part of the iter into right and left halves (fig. 3). The anterior portion of this septum is visible

FIG. 3.



in sections beneath the ciliated grooves in the region of the posterior commissure (fig. 1), while it extends backwards throughout the whole or very nearly the whole length of the mid-brain.

Thanks to the kindness of Dr. S. J. Hickson, F.R.S., Professor of

* 'Berichte über die Verhandlungen der Naturforschenden Gesellschaft zu Freiburg i. B.,' vol. 6, Heft 3, p. 83.

Zoology in the Owens College, I have been able in a striking manner to confirm the results thus obtained in the New Zealand Ammocete by an investigation of the corresponding organs in one of the European species. Professor Hickson kindly placed at my disposal for the purposes of this investigation a series of transverse sections, which had been cut a short while before by his assistant, but which he had not yet examined, and I was delighted to find in these, without the slightest difficulty, the structures which I had previously discovered in New Zealand.

The Owens College Ammocete was, to judge from the size of the sections, considerably older than the New Zealand specimen, and this possibly accounts for certain differences in the arrangement of the parts under discussion. It appears also to have been treated with osmic acid, while the staining was effected by means of iron Brazilin. The columnar epithelium of the ciliated grooves is perhaps not quite in such a good state of preservation histologically as in the New Zealand specimen, but evidently has much the same character. Beneath the posterior commissure the grooves are widely separated from one another (fig. 4) instead of being in close contact. Anteriorly (fig. 5) they are I think better defined than in the New Zealand specimen, and the left one can be traced a good deal further forwards

FIG. 4.

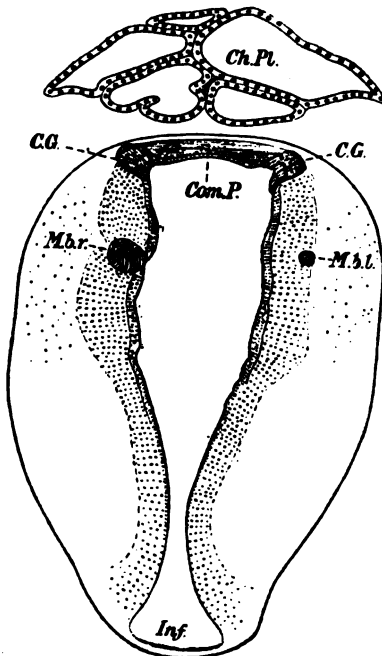
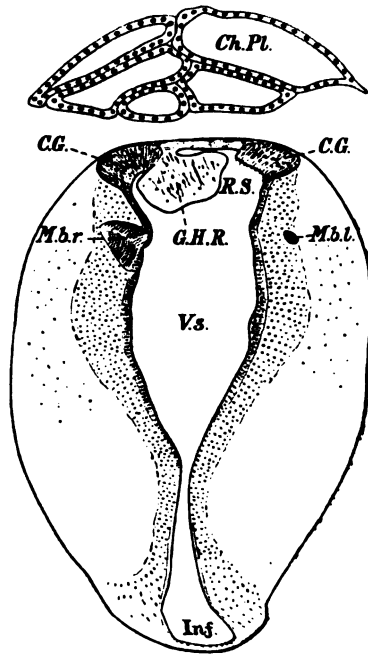
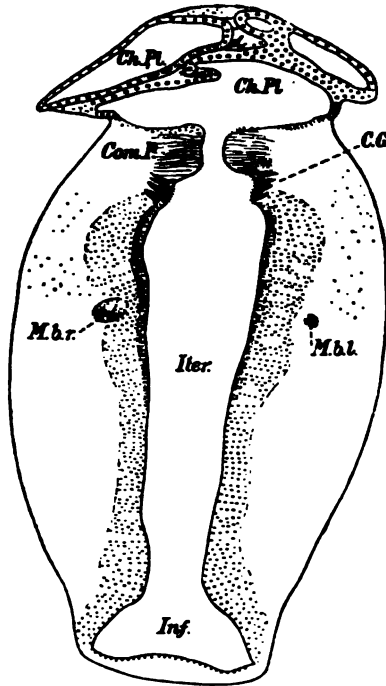


FIG. 5.



than the right, owing apparently to the great development of the right ganglion habenulæ, which projects into the brain-cavity and more or less blocks up the angle between roof and side wall. Posteriorly (fig. 6) they appear to terminate—as grooves—at about

FIG. 6.



the level of the hinder margin of the posterior commissure, but a band of long columnar cells appears to be continued backwards for a short distance after their groove-like character has disappeared, so that we find such a band on either side, bordering the narrow opening between the iter below and the cavity of the choroid plexus of the mid-brain above.

The most striking difference as compared with the New Zealand *Ammocete* concerns the arrangement of the choroid plexus of the mid-brain, which no longer dips down into the iter in the form of a deep, continuous, vertical septum (compare figs. 3 and 6). (Such a septum is, however, simulated in the sections by a mass of granular material containing what look like nuclei irregularly scattered through it. This appearance, which is not represented in the figures, suggests that the septum has undergone degeneration, or possibly the granular mass is simply a coagulum containing the remains of cells shed from

the lining epithelium; it is, however, easily distinguished from the choroid plexus, to which it is attached above, and which has the usual relations. It seems most likely that the vertical septum, if it ever existed in the European form, has simply been withdrawn upwards in the course of development, to take part in the formation of the extensive tela choroidea which forms the roof of the mid-brain.)

The subject can hardly be dismissed without some attempt to explain the presence of the ciliated grooves, and it is by no means difficult to assign to them a probable function. Gaskell, as is well known, has endeavoured to prove that the tubular nature of the central nervous system of vertebrates may be accounted for by its derivation from the alimentary canal of an invertebrate ancestor. He asks,* "On any of the other theories, why is there a nerve tube found with a dilated anterior extremity? Why are the cerebral vesicles formed? Why the choroid plexuses? Why does that tube terminate in the anus? Why is there an infundibular prolongation?" Without attempting to answer all these questions, I venture to hope that the facts recorded in this communication may throw some light upon the function of the cavities of the central nervous system. It has already been suggested that these may aid in the process of gaseous interchange. Thus, H. Blanc "believes that the paraphysis is not a degenerating organ, but is of great importance in connection with the development of the choroid plexus. Like the last-named, it has probably to do with the gaseous interchanges which are carried on in the cavities of the ventricles."† Now gaseous interchange, or respiration in the widest sense of the term, must obviously be a very necessary function for the nerve-cells. These cells in the *Ammocete* lie, as is well-known, in the inner portion of the brain substance, which forms a thick layer, separated from the fluid in the brain-cavity by the lining epithelium. Their relative positions are such that gaseous interchange may take place freely between the fluid in the brain-cavity on the one hand, and the grey matter of the brain on the other; but in order that this interchange may continue effectually two other conditions must be fulfilled: (1) Means must be provided for removing from the fluid in the brain-cavity the waste matters which it takes up from the brain-substance, and in return providing it with fresh supplies of oxygen and perhaps also of dissolved nutriment; and (2) Means must also be provided for maintaining a circulation of the fluid in the brain-cavity itself. The first of these conditions is apparently fulfilled by the vascular choroid plexus, while the second is, I believe, fulfilled in the *Ammocete* to a large extent by the ciliated grooves described above, probably

* 'Journal of Anatomy and Physiology,' vol. 32, p. 545.

† H. Blanc, "Epiphysis and Paraphysis in *Salamandra atra*," 'Arch. Sci. Phys. Nat.,' vol. 10 (1900), pp. 571-2. Abstracted in 'Journal of Royal Microscopical Society,' June, 1901, p. 257.

acting in conjunction with the longer cilia which line other parts of the brain-cavity. The occurrence of cilia in some part or other of the central nervous system of vertebrates has been so often demonstrated* that it is hardly necessary to discuss other cases here, except to point out that their function is probably in all cases to promote the circulation of the brain-fluid.

As to the choroid plexuses of the vertebrate brain there can, I venture to think, be little doubt as to their importance in promoting the oxygenation of the brain-fluid. Especially suggestive from this point of view is the manner in which these plexuses sometimes project into the cavities of the ventricles. A good example of this is afforded by the vertical septum of the choroid plexus of the mid-brain in the New Zealand Ammocoete, as described above. This septum is highly vascular, and it is extremely significant that the ciliated grooves are so arranged as probably to direct a stream of brain-fluid along each side of it. Whether the direction of this stream is forwards or backwards, it is of course impossible to say without direct experiment. Personally, I am inclined to think that it will be found to flow forwards, and that the ciliated grooves may be looked upon as special organs for conveying an abundant supply of oxygenated fluid to the forepart of the brain (especially, perhaps, to the right ganglion habenulæ, which, as is well known, is enormously developed in the Lamprey, and apparently fulfils some important function in connection with the parietal organs). In the young Ammocoete the first choroid plexus, which may be supposed to be especially concerned in the respiration of the fore-brain, is not yet developed; the second and third choroid plexuses, belonging respectively to the mid- and hind-brain, are, on the other hand, already extensive. We need not, therefore, be surprised to find that the fore-brain at this stage is dependent to a large extent for its means of respiration, and perhaps also for its nutrition, upon the choroid plexus of the mid-brain, and that a special apparatus is developed for securing a forward flow of the necessary fluid in the brain-cavity. With regard to this fluid itself it is interesting to note in this connection the recently expressed opinion of Minot:† “The pineal region develops a series of structures, which, from their anatomical characteristics, appear to be directly concerned in the formation of the fluid in the cavities of the brain. We may assume that the choroid plexus supplies the main bulk of the fluid, but the gland-like organisation of the epiphysis and of the paraphysis indicates that they supply by secretion special chemical substances to the encephalic fluid.”

* See for example Klein, ‘Quart. Journ. Micros. Sci.,’ vol. 20, N.S., p. 476; Retzius, ‘Biologische Untersuchungen,’ Neue Folge, vol. 6, p. 59; Beard, ‘Anatomischer Anzeiger,’ vol. 3, p. 902.

† ‘American Journal of Anatomy,’ vol. 1, No. 1, p. 96.

Considering how much attention has of late years been devoted to the study of the Lamprey's brain, it seems almost incredible that such definite structures as the ciliated grooves described above should have hitherto escaped observation. I have, however, been unable to find any reference to them in the literature available. It remains to be seen whether or not they occur in the adult. I am inclined to think that they probably disappear when the development of the first choroid plexus renders their presence no longer necessary, and such circulation of the fluid in the cavity of the brain as is requisite may be safely left to the cilia lining other parts. Ahlborn* figures a transverse section of the brain of *Petromyzon planeri* taken in the region of the posterior commissure, but makes no mention of the structures in question. Gaskell† also figures a transverse section through the corresponding region of the brain of what he terms "an *Ammocetes* immediately after its metamorphosis," without giving any indication of the ciliated grooves. It seems highly probable, then, that they are really absent in the adult animal, and that their loss is to be looked upon as another of those striking changes which accompany the transformation of the larval *Ammocetes* into the adult *Petromyzon* in Europe, or the closely allied *Geotria* in New Zealand.

Much more remarkable than the apparent absence of the ciliated grooves in the adult is the fact that they seem, so far as I have been able to discover, to have entirely escaped the notice of those investigators who have so minutely described the development of the Lamprey. Thus in the beautiful memoirs of Shipley‡ and Scott§ I can find no reference at all to any such structures.

The general appearance and structure of the ciliated grooves forcibly call to mind the occurrence of more or less similar organs in other divisions of the animal kingdom. We may, for example, compare them physiologically with the siphonoglyphs of Alcyonarians, the ambulacral grooves of Echinoderms, and the endostyle of Amphioxus and the Tunicates. It is generally admitted that the function of all these organs is to bring about the movements of fluids in definite directions, and by analogy we may, I think, pretty safely conclude that a similar function is performed in the case under discussion. One is also tempted to institute a physiological comparison with the ciliated tubes in connection with the brain of the Nemertines, but in the present state of our knowledge such a comparison can be of but little value.

* Ahlborn, "Untersuchungen über das Gehirn der Petromyzonten," 'Zeit. Wiss. Zool.', vol. 39, p. 191, Plate 14, fig. 26.

† Gaskell, "On the Origin of Vertebrates from a Crustacean-like Ancestor," 'Quart. Journ. Micros. Sci.', vol. 31, N.S., p. 379, Plate 25, fig. 3.

‡ Shipley, "On some Points in the Development of *Petromyzon fluviatilis*," 'Quart. Journ. Micros. Sci.', vol. 27, N.S., p. 325.

§ Scott, "Notes on the Development of *Petromyzon*," 'Journal of Morphology', vol. 1, p. 253.

494 *On a Pair of Ciliated Grooves in the Brain of the Ammocoete.*

Probably, however, the closest physiological parallel is really to be found in the ciliated groove which, according to Beard, is to be found lying "at the base" of the neural tube in the embryos of nearly all vertebrates,* and which also doubtless serves to promote the circulation of the fluid in the cavity of the central nervous system.

DESCRIPTION OF FIGURES.

All the figures, except fig. 2, were drawn with the aid of the camera lucida under Zeiss objective A, with eyepiece No. 3. Fig. 2 was drawn with the aid of the camera under Zeiss objective D, eyepiece No. 3.

- Fig. 1. Transverse section of the brain of the New Zealand Ammocoete through the region of the posterior commissure.
- Fig. 2. Portion of similar section of the same specimen a short distance in front of the last, showing the histological structure of the ciliated grooves.
- Fig. 3. Transverse section of the same specimen just behind the posterior commissure, showing the ingrowth of the choroid plexus of the mid-brain to form a vertical septum.
- Fig. 4. Transverse section of the brain of the Owens College Ammocoete through the region of the posterior commissure.
- Fig. 5. Similar section through the region of the recessus sub-pinealis.
- Fig. 6. Similar section at about the level of the hinder margin of the posterior commissure.

Explanation of Lettering.—*Ar. T.*, arachnoidal tissue. *C.G.*, ciliated groove. *Ch. Pl.*, choroid plexus. *Com. P.*, posterior commissure. *G.H.R.*, right ganglion habenulæ. *Inf.*, Infundibulum. *Iter*, aquæductus Sylvii or iter. *M.b.l.*, left Meynert's bundle. *M.b.r.*, right Meynert's bundle. *M.C.*, connective tissue brain case (membranous cranii). *R.S.*, recessus sub-pinealis or infra-pinealis. *Tr.*, trabeculæ cranii. *V. 3*, third ventricle.

* 'Anatomischer Anzeiger,' vol. 3, pp. 902, 903.

February 27, 1902.

Sir WILLIAM HUGGINS, K.C.B., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Bakerian Lecture, "The Law of the Pressure of Gases," was delivered by Lord Rayleigh, F.R.S.

The following Paper was read:—

"Note on the Discovery of a New Trypanosoma." By Lieut.-Colonel DAVID BRUCE, R.A.M.C., F.R.S.

BAKERIAN LECTURE.—"On the Law of the Pressure of Gases between 75 and 150 Millimetres of Mercury." By LORD RAYLEIGH, F.R.S. Received January 15,—Read February 27, 1902.

(Abstract.)

The observations here recorded were intended to bridge over in some degree the gap between the very low pressures (below 1.5 mm.) dealt with in a recent paper and pressures approaching the atmospheric for which the usual mercury column and cathetometer method is adequate. The principal novelty consists in the use of two similar manometric gauges. Pressures in the ratio of 1:2 are obtained by the use first of a single gauge and secondly of the two gauges connected in series. The equality of the gauges is tested by observations upon them when combined in parallel. The use of these gauges allows abundant accuracy in the measurement of the pressures, and the difficulties relate rather to the adequate determination of volumes and temperatures.

The results show that, to an accuracy of $\frac{1}{3000}$, air, hydrogen, oxygen, and argon obey Boyle's law. In the case of nitrous oxide a deviation was observed in the direction that might be expected.

"Note on the Discovery of a New Trypanosoma." By DAVID BRUCE, F.R.S., Lieut.-Col., R.A.M.C. Received February 24.
—Read February 27, 1902.

I have received from South Africa specimens of blood taken from cattle which contain a new species of Trypanosoma.

This new species can be at once distinguished from the Trypanosomas of Surra, Tse-tse Fly Disease, or Rat by its larger size, it being almost twice as large as any of the others. In general appearance it conforms closely to the others in possessing an oval protoplasmic body, a longitudinal fin-like membrane, and a single flagellum.

This new Trypanosoma was lately discovered by Dr. A. Theiler, who is in charge of the Bacteriological Laboratory of the Medical Officer of Health, Pretoria, Transvaal.

He states that at first he was under the impression that he had merely hit against the familiar Trypanosoma of tse-tse fly disease. He, however, was struck by its larger size, and tried a few inoculation experiments. He found that the new Trypanosoma only infects cattle. Horses, dogs, goats, rabbits, and guinea-pigs are all immune, neither showing symptoms nor the presence of the parasites in the blood. With the same blood he infected two calves, which showed distinct febrile reaction, and at the same time the parasites appeared in the blood.

He found the parasite for the first time in the blood of a young ox which had just recovered from an attack of rinderpest, and since then has successfully inoculated calves from two other cattle.

He describes the disease as an acute pernicious anæmia with grave blood changes, a general anæmia without deformation of the elements of the blood, or, lastly, only a slight fever, and that there exists a natural immunity in cattle against this Trypanosoma.

He is of opinion that this disease is the same as that attributed by Dr. Kolle—who studied rinderpest in South Africa with Koch during the last outbreak—to bovine malaria. Dr. Kolle overlooked the Trypanosoma, saw that the disease was infectious, and thought he observed endoglobular parasites and pigment in the red blood corpuscles.

As this discovery seems to me to be an interesting one, and as Dr. Theiler deserves great credit for the observation, I would propose that this Trypanosoma be named after the discoverer *Trypanosoma Theileri*.

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END OF THE SIXTY-NINTH VOLUME.





REPORTS
TO
THE MALARIA COMMITTEE
OF
THE ROYAL SOCIETY.

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1902.



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REPORTS, &c., FROM MESSRS. STEPHENS AND
CHRISTOPHERS, BENGAL.

"The Relation of Malarial Endemicity to '*Species*' of Anopheles." By J. W. W. STEPHENS, M.D. Cantab., and S. R. CHRISTOPHERS, M.B. Vict. Received October, 1901.

1. *Definition of Endemicity.*

It has always been recognised that in a particular country certain districts are more malarial than others. If we proceed to ascertain to what extent malaria prevails in a district we may do so in several ways—

(1.) We may consult the hospital statistics for admissions for malaria.

(2.) We may determine to what extent enlargement of the spleen prevails.

(3.) We may determine by actual blood examination how many individuals have parasites in the peripheral circulation. More especially does the parasite rate in children, as pointed out by Koch, give a definite and true index of endemicity, which may be used in the comparison of one locality with another.

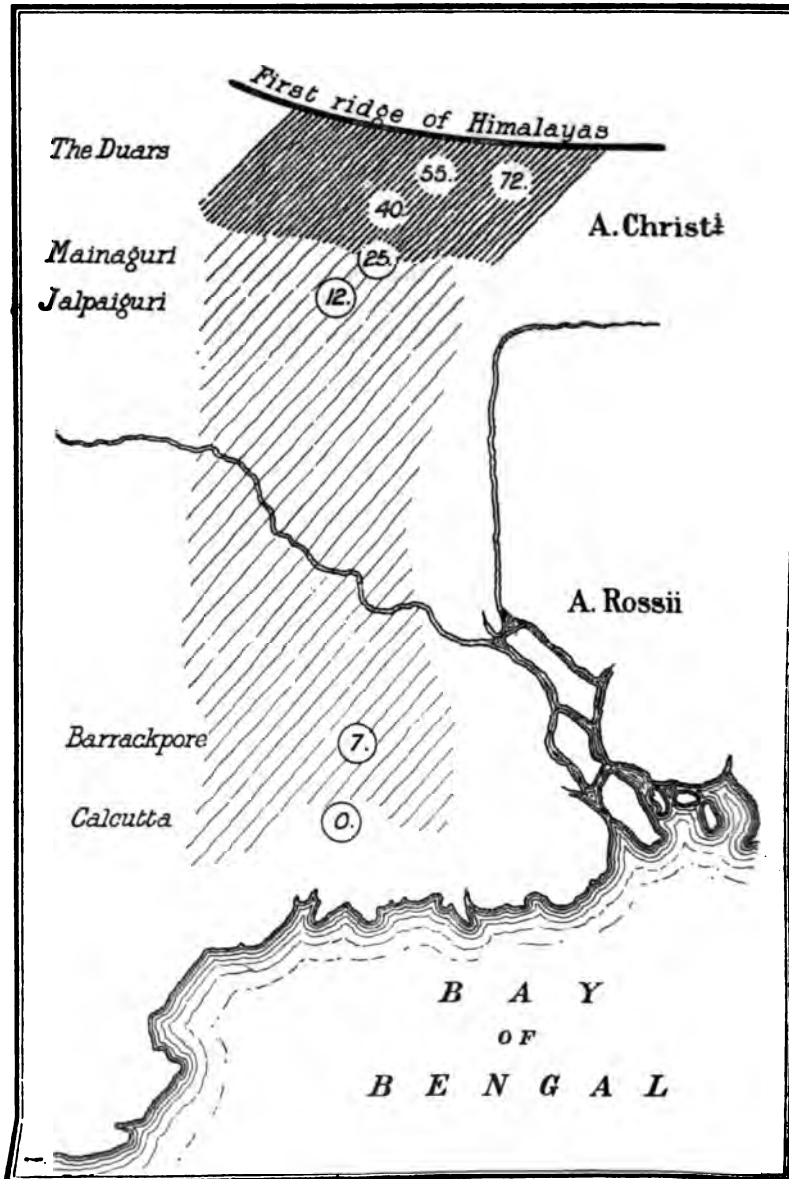
To this method we would add, as a complementary one, the determination of the percentage of infected Anopheles, as giving the actual risk of infection in a district.

These methods of determining the endemicity are by no means of equal value. The two first we shall further discuss in another paper. We may say shortly, however, that the first is of quite secondary importance and open to grave errors, whilst the second, though of value in districts where blood examinations cannot be made, yet is also open to serious error.

It is in the third sense that "endemicity" or "endemic index" is used by us in this paper. The endemic index, then, we may take to be the percentage of infected children (under 10 years of age) in any district, and is the truest test of the infection present and the liability of immigrants to contract malaria. In this way Koch has shown the endemicity of malaria to vary in the Dutch East Indies from 0 per cent. to 100 per cent. Our own researches and those of Annett and Dutton have shown that in West Africa the endemicity of the various districts is uniformly very high—20 per cent. to 100 per cent.

2. *The Malarial Endemicity of Bengal.*

By the examination of a number of children from various districts we have determined the endemicity of representative portions of Bengal. The following map and table will at once show how, in pro-



Sketch Map to show Variation of Endemicity in Bengal.

Table I.—Showing Variations in Endemicity in Bengal.

Locality.	Endemic index.	Number of children examined.
Calcutta { Phoolbagan Road	0 per cent.	57
Hastings	0 "	84
Kidderpore	0 "	50
Plains of Bengal { Barrackpore	7·7 "	26
Belghurria	7·4 "	27
Jalpaiguri	12·7 "	86
Mainaguri	25 "	40
Duars { Rangamutty	43 "	72
Nagasuri	55·5 "	27
Ghatia	72 "	25

ceeding from Calcutta northwards till the foot of the Himalayas was reached—a distance of some 300 miles—we passed from a region the endemic index of which was 0 per cent., through regions with increasing indices of endemicity, till at the foot of the hills, in a district known as the Duars, a very high degree of infection was reached, 40 per cent. to 72 per cent., as high, indeed, as that found by us in West Africa. We have, then, a region, not above 300 miles in latitude, subject to almost identical climatic influences, with an endemic index varying from 0 per cent. to 72 per cent.

3. *The Cause of the Variation in the Endemicity of Bengal.*

As just mentioned, the climatic influences are very similar throughout the district. A high degree of atmospheric humidity, a very uniform high temperature—86° to 90° in the shade—and the presence of abundant surface water. In these respects we could see no essential difference between regions of low and those of high endemicity. We are left, then, with the following possible explanations of the variation:—

- a. The number of *Anopheles* present.
- b. The effect of the importation of non-immunised individuals.
- c. The "species" of *Anopheles* in different districts are not the same.

a. *The Number of Anopheles Present.*—The portions of Calcutta investigated by us were portions of the native town, in both central and outlying districts, which, from their squalid nature, appeared the most likely to possess high fever rates. Here we had, at first sight, all the conditions which, from their similarity to those of West Africa, would have led us to expect a high endemic index. The temperature

during some months had been as high as 86° to 90° in the shade during the day, and the atmospheric humidity was as great as that of the most malarious districts of the West African Coast.

The houses were in no essential particular different from those in Africa, nor was the social condition of the inhabitants higher.

Anopheles were everywhere present, and in some quarters were extraordinarily abundant. Even in Africa we have never seen larvæ so abundant or adult insects so readily caught.

We had, then, here apparently all the essential points of an African village reproduced, and we naturally expected a considerable degree of child infection. But this proved not to be the case.

(1.) An examination of the children of several of the worst districts and those with most Anopheles was completely negative. In all, 141 children were examined throughout June, July, and August without a single parasite being found by us.

We may note that, though during this time many cases of enlarged spleen were admitted into the different civil hospitals of Calcutta and diagnosed as malaria, yet parasites were only found with the greatest rarity. The only hospitals in which parasites were readily found by us were the military hospitals.

(2.) An examination of the Anopheles from these quarters of Calcutta was also completely negative. In all, 324 Anopheles were dissected, and neither sporozoits nor zygotes were found by us.

Table II.—Showing Absence of Malarial Infection in Calcutta.

District of Calcutta.	Endemic index.	No. of children.	Sporozoit rate.	No. of anopheles.
Phoolbagan Road	0 per cent.	57	0 per cent.	200
Hastings... ..	„	77	..	5
Fort William.....	„	7	0 per cent.	116
Kidderpore.....	„	50	..	8
Total.....	..	191	..	329

Briefly, we may say that in Calcutta we had in June, July, and August an extraordinary number of Anopheles with an endemic index of 0°.

A very different state of affairs were present in the Duars. Here, though frequently we had great difficulty in collecting sufficient Anopheles to enable us to determine the sporozoit rate, the amount of malarial infection was very large. Whereas in Calcutta, in two districts at least, we were able to collect in an hour 50 or more Anopheles, in Nagasuri, in the Duars, with an endemic index of 55 per

cent., we collected during many days and with difficulty only 39 specimens.

It is evident, then, that the mere number of *Anopheles* present cannot be the cause of the great variation in the endemicity of Bengal.

b. The Effect of the Importation of Non-immunised Individuals.—Koch has shown how the importation of fresh coolies from the non-malarial districts is responsible for an increase in the fever incidence, and that an immunity is later developed in these, so that a gradually diminishing fever incidence ensues, until a new set of coolies is again imported.

No large immigration occurs in the district under consideration, except on the tea plantations of the Duars. As this district is the one with the highest fever rate, it is possible that the extremely high endemicity may be partly due to this cause. The endemic index taken as the test of each district was, however, determined from the coolies coming from the plains who dwelt in separate villages from the hill coolies. Moreover, the coolies from the plains were for the most part long resident in the district, and most of the children examined by us were either born in the district or had lived nearly the whole of their life there.

There was also already a marked increase in endemicity (25 per cent.) at Mainaguri, about 30 miles from the Duars. Here there is no immigration, and the inhabitants are all long resident in the district.

It seems probable, then, that over and above the effect of the immigration of strangers, there is a real and marked increase in the endemic index in this district.

c. The Species of Anopheles in Bengal.—A closer examination of the *Anopheles* occurring in Bengal proved that we were dealing with a considerable number of species. It also became evident that the distribution of certain species coincided with areas of high endemicity, whilst other species occurred and even existed in profusion where very little infection was present.

The species of *Anopheles* found by us were six in number: *A. Rossii* (Giles), *A. fuliginosus* (Giles), *A. sinensis* (Wied), sub-sp. *nigerrimus* (Giles), *A. Lindesayi* (Giles), and two species new to India. These latter are *A. metaboles* (Theobald) and *A. Christophersi* (Theobald).

It was found that in many parts of Calcutta one species only occurred from June to August, namely *A. Rossii*. This species was present in enormous numbers.

In certain outlying portions of Calcutta, and in the country around Calcutta, two other species were found somewhat sparingly, namely, *A. fuliginosus* and *A. sinensis*, sub-sp. *nigerrimus*.

These three species appear to be present everywhere in the plains of Bengal. They were the only species found by us until the foot of the mountains (Duars) were reached. *A. Rossii* was also found by us at a

height of 5000 feet, *A. fuliginosus* at 800 feet, and *A. nigerrimus* at 1200 feet North of Jalpaiguri, about 30 miles from Duars, *A. metaboles* was found breeding scantily. This species was found to increase in numbers as we passed northwards, and it extended high into the hills (5000 feet).

The district known as the Duars is a strip of land extending for some hundreds of miles along the foot of the Himalayas. It is a gently sloping tract of from 800 feet to 1200 feet above the sea, and abounding in small streams. In this narrow strip another new species, *A. Christophersi*, was found in abundance by us. This is a very small mosquito and resembles to some extent *A. funestus*. We have never found this species in any of the districts visited by us, except in the Duars.

A. metaboles was in the Duars also a very common mosquito, judging by the large numbers and wide distribution of larvæ.

Our observations so far then can be stated briefly as follows :—

Table III.—To show Relation of Species of Anopheles to Endemicity.

Locality.	Species present.	Endemic index.
Calcutta { Phoolbagan Road.... Hastings	<i>A. Rossii</i> . <i>A. Rossii</i> . <i>A. fuliginosus</i> .	0 per cent. 0 per cent.
Plains of Bengal.....	<i>A. Rossii</i> . <i>A. fuliginosus</i> . <i>A. nigerrimus</i> .	From 7 to 12 per cent.
Duars	<i>A. Christophersi</i> . <i>A. metaboles</i> . <i>A. Rossii</i> .	From 40 to 72 per cent.
Hills (5000 feet)	<i>A. metaboles</i> . <i>A. Rossii</i> . <i>A. Lindesayi</i> .	0 per cent.

Such facts led us to question whether considerable variation did not exist in the power of different species of anopheles to act as host to the malarial parasites, and whether the endemicity in any district might not, to a large degree, be dependent on which species of anopheles occurred there.

We therefore wished to determine for each of the species whether, in the first place, it was a carrier of malaria. Unfortunately, in the Duars we had few facilities for feeding, and the evidence for some of the species is not conclusive. The following experiments and observations show, however, that *A. Rossii* appears to be not a carrier, or in any case a very poor carrier, that *A. Christophersi* is a good carrier, and that *A. fuliginosus* and *A. nigerrimus* are doubtful as carriers.

Relation of Malarial Endemicity to "Species" of Anopheles. 9

A. Rossii.—The fact that this species can occur in such profusion as it did in Calcutta without a corresponding degree of infection, is in itself strongly suggestive that this species is a harmless one.

We may note also that Major Ross was unable to cultivate human malaria in Calcutta, where he must have used *A. Rossii*, but that he succeeded in doing so with some specimens of *Anopheles* up country.

Our own experiments with *A. Rossii* have not been more successful. The insects used by us were not reared from larvæ, but caught already fertilised in sheds and houses. Any possible infection in these would in no way vitiate the experiment, and by this means we avoided the risk of non-fertilisation or other influences which render the use of bred insects uncertain.

The following table gives the result of our experiments with *A. Rossii*:—

Table IV.—To show Result of Feeding *A. Rossii* upon Cases of Malaria.

Case.	Date of feeding.	Date examined.	Result.	Number examined.
Simple tertian Numerous large forms.	July 20, 21, 22, 23	July 25	Negative	8
Crescents Very abundant throughout.	July 21, 22, 24	July 26	Negative	5
Simple tertian Numerous large forms.	July 22, 23.....	July 25	Negative	6
Simple tertian Fairly numerous large forms.	July 24, 26.....	July 27	Negative	9

As *A. fuliginosus* and *A. nigerrimus* seemed to be the only alternative carriers of malaria in the regions of low endemicity around Calcutta, as many specimens as possible were examined. In an accompanying paper we have shown that though *A. nigerrimus* is a very common species breeding extensively over large tracts of marsh, yet that in houses it was seldom caught. The difficulty of obtaining specimens prevented our determining whether this species carried sporozoites. A small number (four) caught in houses were fed every night for five nights on a case which had crescents in large numbers. These were examined on the fifth day, but were negative. Eight specimens caught in houses were also negative.

A. fuliginosus were more abundant in houses, but still rather rare. Thirty-six specimens of *A. fuliginosus* were found negative. Two specimens were dissected on the fifth day, after feeding on a crescent case, but were negative.

A. Christophersi.—This species is undoubtedly a good carrier. Sporozoites were found in 4 out of 64 specimens, or 6·25 per cent. This rate is only a little lower than that frequently found by us in Africa.

A. metaboles.—From the difficulty of obtaining the adult insects, sufficient numbers were not examined to determine whether this species carried malaria. Eleven specimens from huts were negative.

It was hoped that sufficient specimens of the five species might have been examined in an area of high endemicity to give figures which would show the sporozoite rate of each species. We were, however, at the present time (August) unable to do so. We believed, however, that marked differences would be found, and we believed this to be the explanation of certain observations made by us in Africa. Thus Lagos had a sporozoite rate of only 3 per cent., while 50 miles up country a sporozoite rate of 50 per cent. occurred. In Lagos we were dealing almost exclusively with *A. costalis*, while in the latter place the common species was *A. funestus*.

Before drawing the conclusion that the endemicity of a district is dependent on the species of *Anopheles* present, much more extended observations than those we have now put forward are required. Even a district like Bengal is in reality so extensive that generalisations are apt to be hasty. We can, however, draw the following conclusions, and they show that the question of "species" is a very important one, and one that may possibly be an important cause in bringing about such varying degrees of endemicity as are found in different districts.

Conclusions.

1. That *A. Rossii* occurs in Calcutta in June, July, and August, together with an endemic index of 0 per cent. and a sporozoite rate of 0 per cent. That it is difficult to cultivate malaria experimentally in this species.

2. That the distribution of *A. Christophersi* corresponds closely with an area of extremely high endemicity. That the sporozoite rate in this species is as high as is usually found in tropical Africa.

We may point out also that the *species of parasite* should also be taken into consideration. It is manifestly not accurate to compare these with one another as simply so many cases of malaria. It is possible that all three parasites do not develop with equal facility in the different species of *Anopheles*. The question of "species" of *Anopheles* in relation to malaria requires to be worked out much more carefully than has yet been done.

"Some Points in the Biology of the Species of *Anopheles* found in Bengal." By J. W. W. STEPHENS, M.D. Cantab., and S. R. CHRISTOPHERS, M.B. Vict. Received October, 1901. (The Determination of the Species by F. V. THEOBALD, M.A.)

In an accompanying Report (The Relation of Endemicity of Malaria to the Species of *Anopheles*) we show that the power of transmitting malaria appears not to be possessed in the same degree by all species, and whilst it is even probable that some species may not be concerned at all in the spread of malaria, the presence of other species is associated with a high rate of malarial endemicity.

It is very necessary, then, that the habits of life of each species of *Anopheles* should be determined, as pointed out by Nuttall, not in general terms but for each particular species.

In the present paper we note such differences as were observed by us in the habits of the species of *Anopheles* which were found by us in Bengal. These were six in number—*Anopheles Rossii* (Giles), *Anopheles fuliginosus* (Giles), *Anopheles sinensis*, sub-sp. *nigerrimus* (Giles), *Anopheles Lindesayi* (Giles), and two new species. These latter resemble *Anopheles costalis* and *Anopheles funestus*, but specimens sent to Theobald were found by him not to be these species. They are described by Theobald as *Anopheles metaboles* and *A. Christophersi*. It was found possible to distinguish both the eggs and larvæ of these species. The eggs of the species under discussion vary more than the larvæ, and, indeed, could sometimes be distinguished by the unaided eye. The eggs of two species—*A. nigerrimus** and *A. Christophersi*—are characterised by an extremely narrow upper surface (see fig. 3, c, d). Those of *A. Rossii* are unique in possessing a broad fringe-like structure arising from the borders of the upper surface (see fig. 3, a).

The larvæ may be distinguished by an examination of the antennæ, of four small hairs hanging over the mouth, of the number of segments bearing palmate hairs, and of the individual leaflets of the palmate hairs. The presence of a large branched hair arising from the inner border of the antenna occurs in only one of the species—*A. nigerrimus*. The four small hairs over the mouth have been noted by Grassi as presenting differences of specific importance.

In the species under discussion the central pair do not present any marked differences. The outer pair, however, may be simple as in *A. Rossii*, *A. metaboles*, or they may be branched as in *A. nigerrimus*, *A. fuliginosus*, and *A. Christophersi*, so as to form a cocade-like tuft over the feeding brushes.

* *A. nigerrimus* is used for brevity; it should read all through as *A. sinensis*, sub-sp. *nigerrimus*. (F. V. T.)

Fully developed palmate hairs are as a rule confined to the last five abdominal segments. A small imperfectly formed hair is also frequently present on the 2nd abdominal segment. In *A. Christophersi*, however, every abdominal segment bears a fully formed palmate hair, and there is an imperfect pair situated on the posterior portion of the thorax. *A. Lindesayi* also has fully developed palmate hairs on the last six abdominal segments.

A palmate hair consists of from 14 to 20 leaflets, arranged so as to form a structure resembling the leaf of a Palmyra palm. The leaflets, towards their distal extremities, are more or less jagged or notched, and, except in *A. nigerrimus*, are continued past these notches only as a filament considerably narrower than the basal portion of the leaflet. The character of the notching and the length of the terminal filament compared with the basal portion were found by us to be of specific nature (see fig. 1).

The following is a description of the main differences in the eggs and larvæ of the species under discussion :—

A. Rossii (see figs. 1, 2, and 3).

Egg.—Anterior surface very broad.

Frill very wide and continued around the edge of the upper surface so that it passes above the floats.

Floats are of the shape of a scallop-shell and reach the border of the upper surface.

Lower surface of egg without distinct polygonal markings.

Larva.—Antenna without large lateral branched hair.

Hairs over mouth unbranched.

Palmate hairs borne by 3rd, 4th, 5th, 6th, and 7th abdominal segments.

Imperfectly formed palmate hair on 2nd abdominal segment.

Palmate hairs rather small. The terminal filament half the length of the whole leaflet.

A. fuliginosus (see fig. 3, b).

Egg.—Upper surface rather narrow.

Fringe rudimentary. Fringe is not continued above floats, being wanting in the portion of the edge of the upper surface where the floats are placed.

Floats regularly oval and touch margin of upper surface.

Lower surface of egg not marked by definite polygonal areas.

Larva.—Antenna without large lateral branched hair.

Outer pair of hairs over mouth developed into a tuft.

Palmate hairs borne by 3rd, 4th, 5th, 6th, and 7th segments.

Palmate hairs dark in colour, very markedly jagged. Terminal filament not so long as basal portion.

A. sinensis, sub-sp. *nigerrimus*. (See figs. 1, 2, and 3.)

Egg.—Upper surface very narrow.

Fringe rudimentary. Passes all round margin of upper surface.

Floats oval.

Floats do not reach margin of upper surface, but leave a portion of lower surface of egg intervening.

Lower surface marked by silvery lines into polygonal areas.

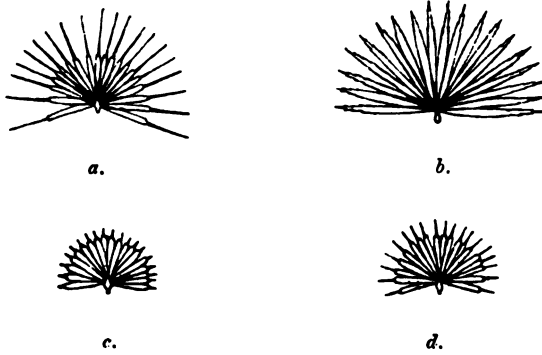


FIG. 1.—Palmate Hairs of Larval *Anopheles*.

a. *A. Rossii*; b. *A. nigerrimus*; c. *A. metaboles*; d. *A. Christophersi*.

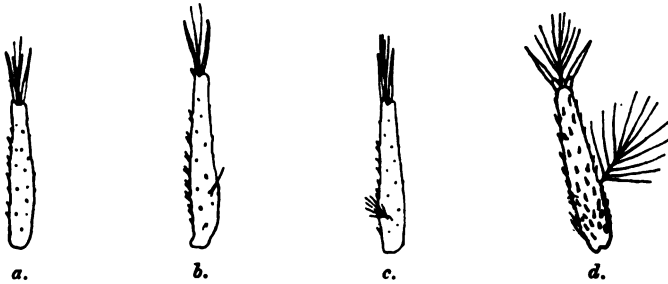


FIG. 2.—Antennæ of Larvæ.

a. *A. Rossii*; b. *A. metaboles*; c. *A. Lindesayi*; d. *A. nigerrimus*.

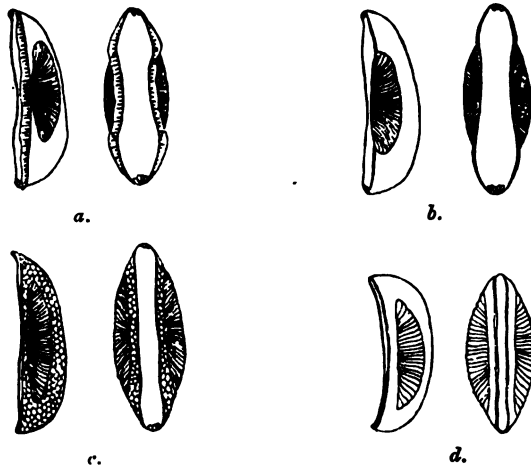


FIG. 3.—Ova of *Anopheles*.

a. *A. Rossii*; b. *A. fuliginosus*; c. *A. nigerrimus*; d. *A. Christophersi*.

Larva.—Antenna with large branched hair arising from inner border.

Outer pair of hairs over mouth developed into large very prominent cocade-like tufts.

Palmate hairs borne by 3rd, 4th, 5th, and 6th abdominal segments. Imperfect palmate hair on 2nd segment.

Palmate hairs very large. Leaflets lanceolate in shape, with the terminal third deeply serrated.

A. Lindesayi.

Egg.—Not observed.

Larva.—Antenna with small rudimentary branched hair. (Fig. 2, c.)

Hairs over mouth unbranched.

Palmate hairs borne by 2nd, 3rd, 4th, 5th, 6th, and 7th abdominal segments.

Palmate hairs large.

Terminal filament long, but not so long as basal portion.

A. metaboles (see figs. 1 and 2, c and d).

Egg.—Not observed.

Larva.—Antenna with rudimentary hair, unbranched.

Hairs over mouth unbranched.

Palmate hairs borne by 3rd, 4th, 5th, 6th, and 7th abdominal segments.

Imperfect hair on 2nd segment.

Palmate hairs rather large.

Terminal filament very short.

A. Christophersi (see figs. 1 and 3, d, d).

Egg.—Egg smaller than in other species.

Upper surface very narrow.

Fringe rudimentary.

Florets do not touch edge of upper surface.

No polygonal markings on the lower surface of the egg.

Larva.—Antenna without large lateral branched hair.

Outer pair of hairs over mouth developed into small tuft.

Palmate hairs borne by all abdominal segments. Imperfect palmate hair on thorax.

Palmate hairs large. Terminal filament rather short, but longer than that of *A. metaboles*.

The distribution of the Species.—*A. Rossii* was found everywhere by us—from Calcutta to the foot of the Himalayas, and at an elevation of 3000 feet. In the Duars it was comparatively rare. In Calcutta, on the contrary, it was by far the commonest species, and in many parts of Calcutta appeared to be the only species present.

A. Christophersi was only found by us in the Duars district, and it was not found above a height of about 1000 feet.

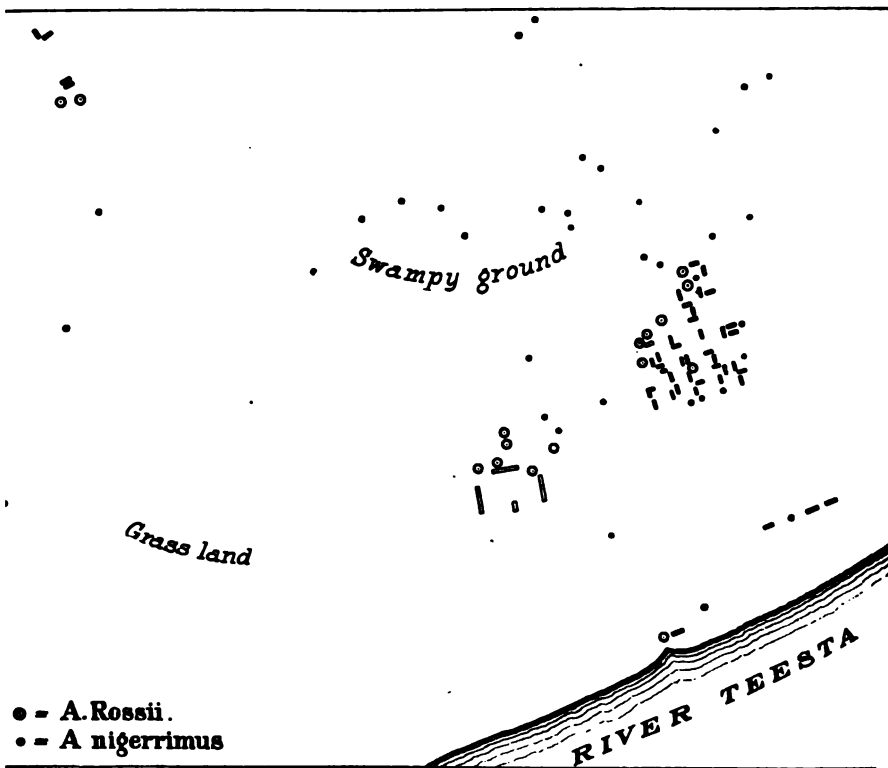
A. metaboles occurred in small numbers as one approached the foot of the hills. It was the commonest Anopheles in the upper portions of the Duars and lower hills. It occurred also at an elevation of nearly 5000 feet near Kurseong.

A. fuliginosus was found scantily in the plains and at a height of 800 feet.

A. nigerrimus was, except in Calcutta, a common species in the plains, and larvæ were found at a height of 1200 feet in the Duars.

Habits of the different Species.

A. Rossii.—*A. Rossii* was found by us always near human dwellings, and often in very foul water. In spite of the commonness of the species, larvæ were never found by us more than a stone's-throw from dwellings. If in any place larvæ were discovered at a greater distance



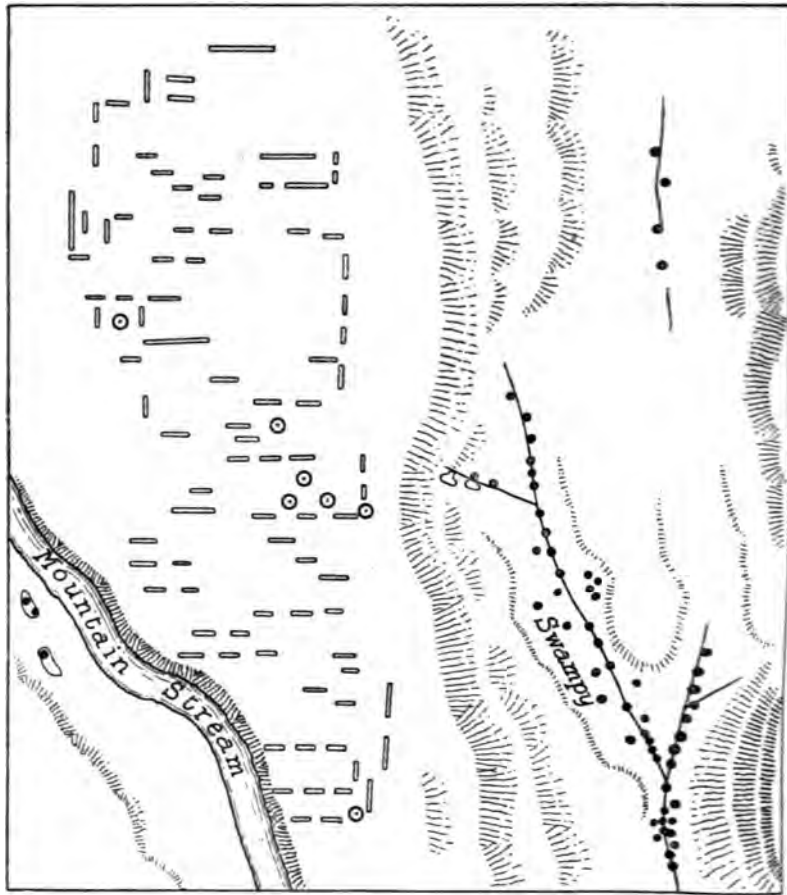
PLAN I.—To show distribution of breeding-places of *A. Rossii* and of *A. nigerrimus*.
Open circles = *A. Rossii*; solid circles = *A. nigerrimus*.

they invariably turned out to be larvæ of other species. *A. Rossii* appears, then, in Bengal to be "foveal" in its distribution, in contradistinction to other species of *Anopheles* to be described.

In Plan I the distribution of *A. Rossii* breeding-places can be compared with those of *A. nigerrimus*, in Plan II with those of *A. metaboles* and *A. Christophersi*.

In captivity *A. Rossii* lays eggs more readily than *A. fuliginosus* or *A. nigerrimus*. The eggs laid on mud were sometimes deposited in a heaped up mass, in others scattered over the surface of the mud. Some of the eggs of *A. Rossii* do not become black, and these appear

to dry up quickly and do not develop. Sometimes the whole of the eggs laid are of this nature, at others a certain number are normal, whilst others remain white or change only to a reddish tinge.



○ = *A. Rossii*.
● = *A. Christophersi* and *A. metaboles*.

PLAN II.—Portion of Coolie Lines on a Tea Plantation—to show breeding-places of *A. Rossii*, *A. Christophersi*, and *A. metaboles*.

Open circles = *A. Rossii*; solid circles = *A. Christophersi* and *A. metaboles*.

The eggs of *A. Rossii* possess very slight powers of resisting desiccation. Gravid females of *A. Rossii* were allowed to deposit eggs upon moist mud in a large earthenware vessel. The mud was then allowed to dry out of doors. From time to time portions of mud were removed and the eggs floated on to the surface of water in such

a way as not to wet them. At the end of 24 hours the surface of the mud was almost dry. Eggs removed and placed in water hatched in a few minutes. At the end of 48 hours the mud was quite dry. Eggs removed and added to water hatched in a few minutes. On the third day eggs removed and added to water did not hatch. On the fourth day the whole mass, with many hundreds of eggs upon its surface, was carefully moistened, but no larvæ hatched out. There does not appear to be, then, any probability of the eggs of *A. Rossi* resisting even a short period of drought in the course of nature.

The larvæ of *A. Rossi* usually occur in very large numbers in the pools they frequent. Other species do not seem to breed in such a crowded state. The food of the larvæ varied somewhat in different situations. In all there was much sand. In larvæ from shallow newly formed rain pools on mud, the gut contents were largely composed of bacilli and small infusoria. In water of a more permanent nature, algæ of several species, and a great variety of unicellular green vegetable cells, were found. Diatoms, though present, rarely formed a large proportion of the food.

The larvæ of *A. Rossi* appear unable to resist drying, even for a few hours. A number allowed to remain on drying mud very quickly died.

Experiments were made to determine how far *A. Rossi* selected a particular kind of water for the deposition of ova. A number of gravid females were placed in a large mosquito net along with earthenware vessels containing different waters. It was found that very little if any choice was shown between pure water, green stagnant water, or even very foul sewage, eggs having been laid in the same night in each of these vessels.

A. Rossi will readily lay eggs on half dry or dry mud, and confined in a dry tube or bottle will lay eggs upon glass.

A few observations were also made on the natural enemies of *Anopheles* larvæ. While some fish (*e.g.*, a small fish which was identified by Major Alcock, I.M.S., F.R.S., as a young carp, and which occurred abundantly in the larger tanks in Calcutta) feed voraciously on larvæ, others do not eat any.

The following table will give a rough idea of the rate at which the commoner water creatures devour larvæ:—

	Number of larvæ added.	Number after 1 hour.	Number after 12 hours.
Large libellula larva	20	7	1
Small "	12	7	0
Beetle (Dytiscidæ)	12	9	2
* Beetle (Clavicorn)	9	9	8
Small dytiscid larva	8	4	2
Coryxa	9	3	0
Nepa	16	5	2
* Tadpole (small species)	9	9	8

From 50-100 larvæ were added to large earthenware vessels containing one or two specimens of the following, which are the commonest tank fish :—

Fish.	Observation.
2 Catfish	Larvæ gradually disappeared during several days.
6 Young barbus	Larvæ all disappeared in 15 minutes.
8 Trichogaster	Larvæ slowly diminished.
4 Polycanthus	Very few larvæ eaten.
2 Small carp (adult) ..	Larvæ apparently undiminished at end of 48 hours.
A small acclimatised salt-water fish	Larvæ not eaten.

Twelve specimens of the young barbus were then added to a small cement tank containing numerous larvæ. At the end of 24 hours no larvæ were visible, and the pool remained free for 6 days, the period during which it was under observation.

It is evident that these small fish devoured larvæ far more rapidly than the other fish used, and than any of the insect enemies. This is a common fish, and is easily caught. It is, moreover, a fish not readily killed by foul water or transportation. For tanks and small collections of water, which it is undesirable to do away with, the introduction of a number of these fish would appear to be at least worth a trial. With regard to experiments with *A. Rossii* we can say :—

1. Little or no selection of water is exercised by the female in laying her eggs.
2. The eggs do not stand desiccation for more than 48 hours.
3. The larvæ are still more susceptible to desiccation.
4. Of small water animals, larvæ of libellula, beetles of carnivorous habits and their larvæ, coryxa and nepa all feed upon larvæ.

* The disappearance of a single larva after so many hours was probably accidental, as larvæ of various sizes were used.

5. Some species of fish are much more voracious feeders on larvæ than others.

A. nigerrimus.—While *A. Rossii* is foveal, *A. nigerrimus*, on the other hand, is non-foveal. Though the insects were seldom caught by us in houses, yet it is a common species, and the larvæ, unlike those of *A. Rossii*, are to be found plentifully in sluggish streams, river banks, marshes, and swampy pools.

This species was at first thought by us to be a somewhat rare species, as it seldom occurred in collections of *Anopheles* made in different quarters. On examining natural waters, however, we found the larvæ to be very prevalent.

Unlike the larvæ of *A. Rossii*, the larvæ of this species occur singly, and can only be detected by frequent "dipping" amidst the grass and water plants. Taking into consideration the great extent of waters that *A. nigerrimus* larvæ were found in, this species is, in the marshy parts of Bengal, one of the commonest.

The larvæ of *A. nigerrimus* were found apart from collections of huts, and though they occurred in deep water, even in the midst of villages, yet they were not more numerous in these situations than elsewhere. It is noteworthy that in one situation, where deep ditches with clean stagnant water were situated side by side with shallow foul puddles, the larvæ of *A. Rossii* were never found in the former but were numerous in the latter, whereas those of *A. nigerrimus* occurred only in the former. Although the adults of this species were only rarely found in houses, yet it readily fed upon human blood. The habits of this species therefore require elucidation.

A. Christophersi.—In the Duars this species breeds in sluggish streams with grassy edges. It was never found by us in puddles in the coolie lines, or in small pools on roads, paths, &c. In some cases larvæ were dipped up from water running with considerable velocity, but always amongst the grass at the edge. The adults were found in the coolie huts, but were difficult of detection from their small size. It is possible that this species has a more powerful flight than *A. Rossii*, as the larvæ usually occurred some distance from the huts. In one case the nearest breeding place was 200 yards away.

A. metaboles.—The larvæ of this species also occur in streams more or less rapid. They are also abundant in swampy tracts by the side of streams, in rice fields, and small pools. They are not found in the small foul pools in the coolie lines. They occur at a height of 5000 feet. This species has been found by us at very considerable distances from habitations.

A. fuliginosus.—In Fort William, Calcutta, the insects were common. Elsewhere we have very seldom caught the adults. The larvæ were found in the same situations as those of *A. nigerrimus*, but they were not so numerous.

A. Lindesayi.—This species was only found by us in the hills at a height of about 4000 feet. It was breeding together with *A. metaboles*.

Summary.

1. *A. Rossii* is a foveal species and will breed in foul water.
2. *A. nigerrimus* occurs quite apart from habitations. It only breeds in marsh water.
3. *A. metaboles* occurs from the base of the hills to 5000 feet. It breeds in pools, swampy edges of streams, and rice fields. It has been found by us at great distances from dwellings.
4. *A. Christophersi* is, in the Duars, almost confined to small streams. It does not extend very high into the hills, and in the part of Bengal seen by us is present only along the base of the hills.

"The Relation between Enlarged Spleen and Parasite Infection."

By J. W. W. STEPHENS, M.D. Cantab., and S. R. CHRISTOPHERS, M.B. Viet. Received October, 1901.

Discussions have recently arisen in scientific literature on the value of the so-called "spleen test" for malaria. Arguments based on this test appear to us not conclusive; because, so far as we know, no data have hitherto appeared in which the spleen rate and parasite rate have been simultaneously determined. While engaged, then, in determining the parasite rate or endemic index of various localities in Bengal, we took the opportunity of determining the spleen rate among the children examined. The data thus simultaneously acquired, and the conclusions to which they lead us, form the subject of this Report.

It is well known that intense malaria may exist without any corresponding affection of the spleen. Thus, F. Plehn,* speaking of enlarged spleen, says: "Sie ist in der überwiegenden Mehrzahl der Fälle nicht grösser als in den von mir in Deutschland beobachteten Fällen von Abdominaltyphus, häufig wesentlich kleiner"; and Danielst† "concludes that as a test for the prevalence of malaria the spleen test may be worse than useless, unless race and age are taken into account." Ross, we believe, found in Freetown, Sierra Leone, very few enlarged spleens amongst the troops suffering from malaria there.

But in India there exists little doubt that amongst natives and Europeans enlarged spleen is one of the commonest occurrences in those suffering, or who have suffered, from malaria. For in the wards

* 'Die Kamerun-Küste,' p. 97.

† 'Thompson-Yates Reports,' 1901.

of the large native hospitals in Calcutta, among the large number of "fever" cases (in July), the great majority were classified as suffering from "malarial cachexia and enlarged spleen." These cases were briefly characterised by anæmia, with an enlargement of the spleen varying from 2—3 fingers' breadth to that of a spleen filling the whole of the left side of the abdomen, and reaching to the pubes; and these cases were quite common. These cases were also not infrequently accompanied by a temperature which oscillated in an irregular way between 99° and 101°, and which might persist for weeks. Quinine or cinchona, as far as we could see, frequently exerted no influence on the course of the temperature.

When we came to examine these cases microscopically, they consistently showed a complete absence of parasites. We examined over 80 of such cases, and in none did we find parasites, or pigmented leucocytes, or any mononuclear leucocyte increase such as we have shown in earlier Reports to be characteristic of a recent infection.

Many of these patients were taking cinchona, but others were not; and in neither were these subsidiary signs of malarial infection—pigment and a mononuclear increase—present.

Further, we had the opportunity of making *post-mortem* examinations on six cases, and in none of these were parasites found in the bone marrow or spleen; though pigment was present in variable amount.

We do not consider these to be new facts, for similar ones have been recorded by Kelsch and Kiener, Ross and Daniels, under the designation "symptomatic fever." We here wish chiefly to point to the importance of these cases from the point of view of classification. It can be of little value to classify cases of this kind (so-called malarial cachexia) with those of, say, simple tertian with numerous parasites in the circulation, under the general heading of malaria (ague or remittent fever). This classification, however, prevails in India; and for this, among other reasons, hospital and dispensary statistics are of little if any value. It can only be by a microscopic differentiation of cases according to the variety of parasite that a true classification can be founded.

In Calcutta, then, we had a series of 80 cases of enlarged spleen without any corresponding parasite infection. No doubt they represented past cases of actual infection with parasites; but, as we shall soon see, they tell us little, though swelling the hospital statistics, of the actual malarial endemicity of Calcutta. In an accompanying Report we have taken the test of the endemicity of malaria, as first shown by Koch, to be the percentage of children (under 10 years of age) infected with parasites. With the endemic index thus obtained we compare, in the following table, the spleen rate:—

Table I.—To show relation between Spleen Rate and Parasite Rate.

Locality.	Spleen rate.	Endemic index.	No. of children examined.
Calcutta	0	0	191
Jalpaiguri--			
Bustee children.....	27	16·1	37 and 62
School.....	14·7	0	210 and 80
Babu children.....	14·2	0	21 and 21
Mainaguri	74	25	51 and 41
Rungamutty.....	83	43·6	72
Sam Sing (2000 ft.)	7·1	16	14
Kurseong I (5000 ft.) ..	0	0	25
Kurseong II (2000 ft.)..	·2	0	20

We see that there is, with some exceptions to be noted, a fair correspondence between enlarged spleen and endemic index from a spleen rate of 0 at Calcutta, with an endemic index of 0, to a spleen rate of 83 at Rungamutty, with an endemic index of 43·6. Further, we see that, with one exception, the spleen rate is higher than the endemic index. The reason of this is, no doubt, that those children who at one age-period have suffered from malaria and enlarged spleen, still have the enlarged spleen at a later age-period, when the actual parasite rate is less.

So in adults we frequently have a number of enlarged spleens without any corresponding parasite infection, as in the case of 19 enlarged spleen cases in Jalpaiguri Jail, none of which showed parasites.

The spleen rate taken without modification is not a true test of the presence of malaria. Moreover, in making comparisons it is necessary that rates for corresponding age-periods should be compared.

An analysis of Table I gives the following results :—

Table II.

	Age.	Spleen rate.	Endemic index.	Number of children.
Jalpaiguri— Bustee children..	1— 5	12·5	0	8 and 17
	6—10	10·5	16·6	19 and 24
	11—15	62·5	18·7	8 and 16
Mainaguri	1— 5	52·3	37·5	21 and 16
	6—10	85	26·6	20 and 15
	11—15	100	0	10 and 10
Rungamutty	1— 5	77·4	43·3	31
	6—10	81·5	39·4	38

With one exception, then, we see that with increasing age the endemic index decreases (this is constantly the rule as found in West Africa), yet the spleen rate goes on increasing.

A factor beyond those already mentioned may cause a variable spleen rate, namely, the kind of parasite. In India we have found the quartan most commonly in the native children examined by us. It is also generally agreed that the simple tertian causes greater enlargement of the spleen than the malignant tertian.

The following conclusions may, we think, be drawn from these facts :—

1. A high endemic index may exist without any appreciable spleen rate (Africa).
2. A high spleen rate may exist in adults without a corresponding parasite infection.
3. In India (Bengal) among children a high spleen rate is a fair indication of the parasite infection.
4. The spleen rate, unlike the parasite rate, increases up to a certain age limit, and may be considerable when the parasite rate is nil.

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